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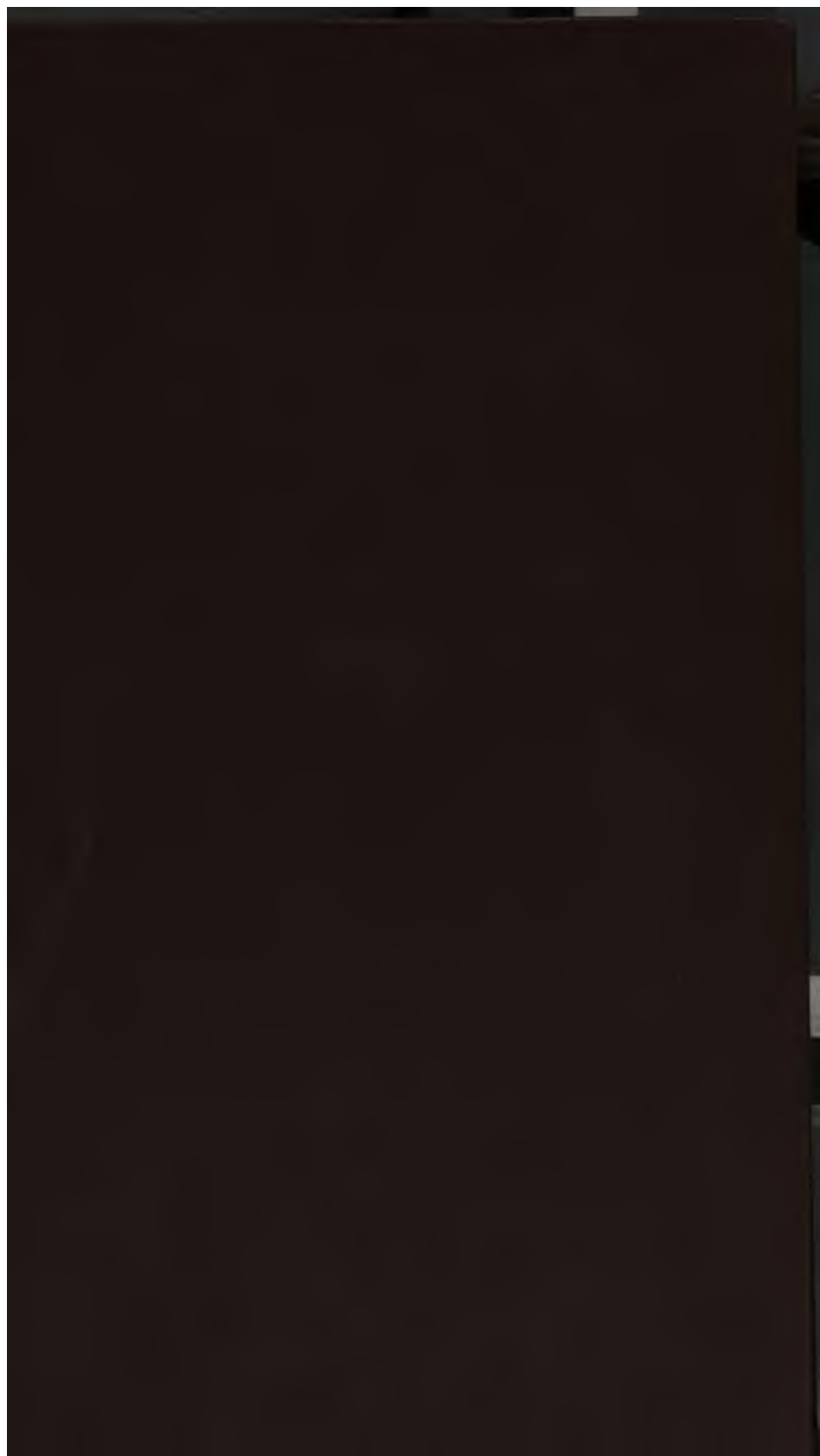
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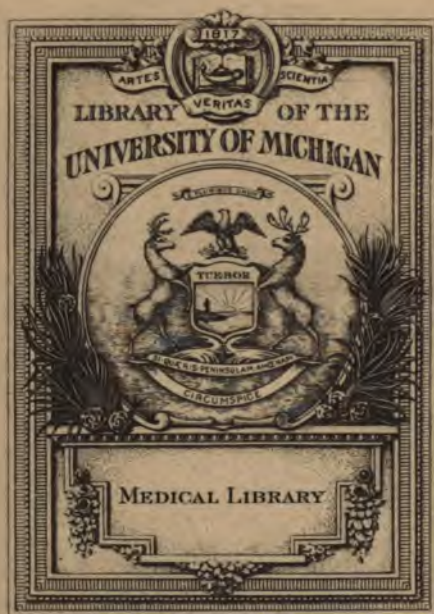
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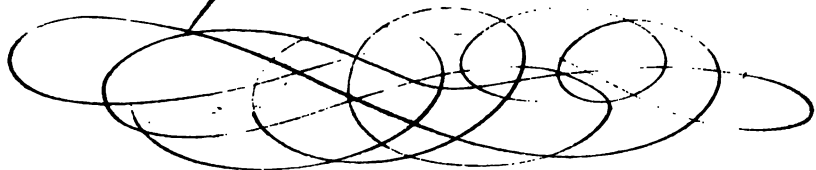








Joseph Hartshorne's



Sept

24

THE  
ECLECTIC REPERTORY,  
AND  
ANALYTICAL REVIEW,  
Medical and Philosophical.

EDITED BY A SOCIETY OF PHYSICIANS.

..... Apis matine  
More modoque.—HOR.

Nullis unius discipline legibus adstricti, quibus in philosophia necessario paremus, quid sit in quaque re maxime probabile semper requiremus.—CIC.

VOL. I.

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1811.



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☞ The following Explanation should have accompanied the Plate which illustrates the Case of Aneurism of the Femoral Artery, (page 506), communicated by Dr. Parrish; but was inadvertently omitted. The reader's attention to this circumstance when examining the drawing is requested.

#### EXPLANATION OF THE PLATE.

AA, the Femoral Artery; its superior part turned on one side in order to show

B, a Groove in the tumor, formed by the pressure of the artery.

C, a Lateral Incision in the tumor, showing the coagulated lymph with which it is filled.

EE, designed to represent a portion of the Os Femoris, to which the tumor was attached.



THE  
ECLECTIC REPERTORY  
AND  
ANALYTICAL REVIEW,  
MEDICAL AND PHILOSOPHICAL.

Edited by a Society of Physicians.

PUBLISHED BY EDWARD EARLE, PHILADELPHIA.

OF all the means which have been devised to disseminate information, that of a Periodical Journal is, perhaps, the best adapted. The utility of such publications is, indeed, very decisively evinced by the encouragement which they receive among every people at all distinguished by an attachment to letters. It is however in the United States, where access to the stores of learning is impeded by peculiar obstacles, that they seem to be more especially required, and to hold out the greatest advantages. Easy of circulation, they reach the remotest portions of our widespread territory, and open at a very moderate expense, a source of amusement and instruction which to many would otherwise be denied. Nor is the salutary influence of these popular vehicles any longer with us, than in Europe, a matter to be determined by experiment, or the results of the future. Effects, the most beneficial to our literary relations, can already be traced to their rapid multiplication, and extensive dispersion. They have insinuated into the minds of our people, who were once, confessedly, too negligent of liberal pursuits, a love of knowledge, and have made us, what in the language of a celebrated foreigner we have truly become of late, one of the most *inquisitive and reading nations of the earth.*

## PROSPECTUS.

Dispositions in every view so auspicious, were to be anticipated from the diffusion of writings so eminently calculated to captivate general attention, and to delight or improve every description of readers, every class of society, every variety of taste, and every gradation of intellectual capacity.

As its title indicates, the Journal now offered to the patronage of the public, will be conducted chiefly on the principle of *selection*. The leading feature of the plan, more distinctly enunciated, is to present, at stated intervals, a sort of synopsis of the foreign Magazines, Journals, and Reviews appropriated to Medicine and its kindred sciences.

Whatever may be thought of the humility of this design, no one will doubt of its importance who is conversant with the periodical works of Europe. As is incident to such publications, the matter which they contain is altogether of a mixed nature, and of merits the most unequal. They are a vast field, where golden wheat is too often choked by useless or pernicious tares, and where it imports us, like the prudent husbandman, to reject the one, and hoard the other. These publications are, moreover, at this time, so numerous, so costly, and so difficult to be procured, that really they can only be consulted, by a large majority of the cultivators of science in the United States, through a medium somewhat similar to the one now proposed. It is therefore meant to give an extract so copious of their contents, as not only to exhibit the progress of the physical sciences abroad, but to do away, in a great degree, the necessity of recurring to the works themselves.

This Journal however is not wholly to be restricted to the province of selection. A department, sufficiently spacious, will be reserved for *original* contributions. But as eclectics, the editors must be permitted here also to exercise the right of choice, and without arrogance or captiousness, of excluding such articles as they may deem unworthy of preservation. They will cordially receive, and conspicuously insert, reports of interesting cases of disease, well written histories of epidemics, accurate meteorological observations, experimental inquiries, ingenious disquisitions, notices of new discoveries, acute but temperate analyses of books; and, in short, whatever in their estimation, may have a tendency to enlarge the sphere of science; and more particularly, to rectify the errors, amend

## **PROSPECTUS.**

the practice, and increase the usefulness of that branch to which they are professionally devoted.

The contents of the Journal will be arranged under the subsequent divisions:

- |  |  |
|--|--|
| 1. Selected Papers.                        | 4. Original Papers.                                |
| 2. Selected Reviews.                       | 5. Original Reviews.                               |
| 3. Medical and Philosophical Intelligence. | 6. List of New Publications, foreign and domestic. |

## **CONDITIONS.**

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II. The price of the work to be three dollars a year, to be paid on delivery of the second number.

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THE  
ECLECTIC REPERTORY,  
AND  
ANALYTICAL REVIEW.

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VOL. I.

OCTOBER, 1810.

No. I.

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SELECTED PAPERS.

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*Observations on the Distemper in Dogs.*

BY EDWARD JENNER, M. D. F. R. S.

From the Medical and Chirurgical Transactions.

THAT disease among dogs which has familiarly been called "the distemper," has not hitherto, I believe, been much noticed by medical men. My situation in the country favouring my wishes to make some observations on this singular malady, I availed myself of it during several successive years, among a large number of fox hounds belonging to the earl of Berkeley; and from observing how frequently it has been confounded with hydrophobia, I am induced to lay the result of my inquiries before the Medical and Chirurgical Society.—It may be difficult, perhaps, precisely to ascertain the period of its first appearance in Britain. In this and the neighbouring counties, I have not been able to trace it back much beyond the middle of the last century; but it has since spread universally. I knew a gentleman who, about forty-five years ago, destroyed the greater part of his hounds, from supposing them mad, when the distemper first broke out among them; so little was it then known by those the most conversant with dogs. On the continent, I find it has been known for a much longer period. It is as contagious among dogs as the small-pox, measles, or scarlet

fever among the human species; and the contagious miasmata, like those arising from the diseases just mentioned, retain their infectious properties a long time after separation from the distempered animal. Young hounds, for example, brought in a state of health into a kennel where others have gone through the distemper, seldom escape it. I have endeavoured to destroy the contagion, by ordering every part of a kennel to be carefully washed with water, then white-washed, and finally to be repeatedly fumigated with the vapour of marine acid; but without any good result.

The dogs generally sicken early in the second week after exposure to the contagion. It is more commonly a violent disease than otherwise, and cuts off, at least, one in three that is attacked by it. It commences with inflammation of the substance of the lungs, and generally of the mucous membrane of the bronchiæ. The inflammation at the same time seizes on the membranes of the nostrils, and those lining the bones of the nose; particularly the nasal portion of the ethmoid bone. These membranes are often inflamed to such a degree, as to occasion extravasation of blood, which I have observed coagulated on their surface. The breathing is short and quick, and the breath is often fetid. The teeth are covered with dark looking mucus. There is frequently a vomiting of a glary fluid. The dog commonly refuses food, but his thirst seems insatiable, and nothing seems to cheer him like the sight of water. The bowels, though generally constipated as the disease advances, are frequently affected with the diarrhœa at its commencement. The eyes are inflamed; and the sight is often obscured by mucus secreted from the eye-lids, or by opacity of the cornea. The brain is often affected as early as the second day after the attack. The animal becomes stupid, and his general habits are changed. In this state, if not prevented by loss of strength, he sometimes wanders from his home. He is frequently endeavouring to expel, by forcible expirations, the mucus from the trachea and fauces, with a peculiar rattling noise. His jaws are generally smeared with it, and it sometimes flows out in a frothy state, from his frequent champing. During the progress of the disease, especially in its advanced stages, he is disposed to bite and gnaw any thing within his reach. He has sometimes epileptic fits, or quick successions of general, though



slight convulsive spasms of the muscles. If the dog survives, this affection of the muscles continues through life. He is often attacked with fits of a different description. He first staggers, then tumbles, rolls, cries as if whipped, and tears up the ground with his teeth and fore feet. He then lies down senseless and exhausted. On recovering he gets up, moves his tail, looks placid, comes to a whistle, and appears in every respect much better than before the attack. The eyes, during this paroxysm, look bright, and unless previously rendered dim by mucus, or opacity of the cornea, seem as if they were starting from the sockets. He becomes emaciated, and totters from feebleness in attempting to walk, or from a partial paralysis of the hind legs. In this state, he sometimes lingers on till the third or fourth week, and then either begins to show signs of returning health (which seldom happens when the symptoms have continued with this degree of violence) or expires. During convalescence, he has sometimes, though rarely, profuse hæmorrhage from the nose. When the inflammation of the lungs is very severe, he frequently dies on the third day. I knew one instance of a dog's dying within twenty-four hours after the seizure, and in that short space of time the greater portion of the lungs was, from exudation, converted into a substance nearly as solid as the liver of a sound animal. In this case, the liver itself was considerably inflamed, and the eyes and flesh universally were tinged with yellow, though I did not observe any thing obstructing the biliary ducts. In other instances, I have also observed the eyes looking yellow.

The above is a description of the disease in its severest form; but in this, as in the diseases of the human body, there is every gradation in its violence. There is also another affinity to some human diseases, viz. that the animal which has once gone through it, very rarely meets with a second attack. Fortunately, this distemper is not communicable to man. Neither the effluvia from the diseased dog, nor the bite, have proved in any instance infectious; but as it has often been confounded with canine madness, as I have before observed, it is to be wished that it were more generally understood; for those who are bitten by a dog in this state, are sometimes thrown into such perturbation, that hydrophobic symptoms have actually arisen from the workings of the imagination. Mr. John Hunter used

to speak of a case somewhat of this description in his lectures.\* Having never, to a certainty, seen a dog with hydrophobia, I am of course unable to lay down a positive criterion for distinguishing between that disease and the distemper, in the precise way I could wish; but if the facts have been correctly stated, that in hydrophobia the eye of the dog has more than ordinary vivacity in it, and as the term implies, he refuses to take water, and shudders even at the sight of it, while in the distemper he looks dull and stupid, is always seeking after water, and never satisfied with what he drinks, there can be no loss for a ready discriminating line between the two diseases.

March 21, 1809.

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*Two cases of Small-Pox Infection communicated to the  
Fœtus in Utero under peculiar circumstances,*

With additional Remarks.

BY EDWARD JENNER, M. D. F. R. S.

From the Medical and Chirurgical Transactions.

IN my second† and third treatises‡ on the vaccine disease, I endeavoured to call the attention of my readers to some physiological facts respecting the nature of the small-pox infection which I considered of great importance, foreseeing that they would eventually become connected with vaccination.

My own observations, and the accumulated evidence of others, have furnished me with numerous instances of incontestible, and sometimes violent symptoms of small-pox occurring in persons who had previously gone through the disease, either in the natural way or by inoculation, mildly or severely.

\* A gentleman who received a severe bite from a dog, soon after fancied the animal was mad. He felt a horror at the sight of liquids, and was actually convulsed on attempting to swallow them. So uncontrollable were his prepossessions, that Mr. Hunter conceived he would have died, had not the dog which inflicted the wound been fortunately found and brought into his room in perfect health. This soon restored his mind to a state of tranquillity. The sight of water no longer affected him, and he quickly recovered.

† Further observations on the Variolæ Vaccinæ, published in 1799.

‡ A continuation of Facts and Observations on the Variolæ Vaccinæ, published in 1800.



The skin we know is ever ready to exhibit, though generally in a very limited degree, the effects of the poison when inserted there, and how frequently do we see eruptions on persons much exposed to the contagion, and these sometimes preceded by sensible illness! Yet should any thing like an eruption appear, or the smallest degree of indisposition occur on the insertion of the variolous matter in those who have gone through the cow-pox, my assertions respecting the peculiarities of the disease might be unjustly discredited.

In the publications above mentioned, I have given the particulars of several instances of severe disease from variolous contagion, occurring in persons who had incontestibly gone through every stage of small-pox at a former period. The subjects of three of these cases were medical practitioners, two of whom had inoculated themselves by way of experiment, and the other received the disease by the contagious effluvia of a patient, in the course of an assiduous attendance. In another case, the subject was a nurse-maid, who also caught the disease by effluvia; and in another, extracted from the Memoirs of the Medical Society, (which is one of the most remarkable cases on record) the patient had the small-pox a second time with such severity as proved fatal, though the first attack had been of the most malignant kind.

These circumstances have led me to entertain the opinion, that the susceptibility to receive variolous contagion always remains through life, but under various modifications or gradations, from that point where it passes silently and imperceptibly through the constitution (as is frequently the case with cow-pox) up to that where it appears in a confluent state, and with such violence as to destroy life.

It is only under particular circumstances, that any proof of the presence of small-pox can be adduced in those cases, in which it passes through the frame without producing eruptions, or in any perceptible degree disturbing the animal functions. Such proof however, is afforded by the obvious infection of the fætus before birth communicated through the mother, herself being already secure from any visible occurrence of the disorder.

The following remarkable cases will exemplify this fact.

About five years ago, I was requested by Dr. Croft to vaccinate the infant of Mrs. W. a lady in Portland Place. The vaccine fluid, which was inserted fresh from the arm of another infant, produced scarcely any effect beyond a little efflorescence on the part, which in a few days disappeared. On expressing my surprise at this, such an occurrence happening very rarely, Mrs. W. soon removed my embarrassment, by the following narrative.

A few days previous to her confinement, she met a very disgusting object, whose face was covered with the small-pox. The smell and appearance of the poor creature affected her much at the time; and though she mentioned the circumstance on her return home, she had no idea that her infant could suffer from it, having had the small-pox herself when a child. During a few days after its birth, the little one seemed quite well, but on the fifth day it became indisposed, and on the seventh the small-pox appeared. The pustules, which were few in number, matured completely. Dr. Croft, who attended her, being curious to know the effect of inoculation from one of the pustules, put some of the matter taken from them, into the hands of a gentleman eminently versed in that practice, which produced the disease correctly. Mrs. W. was not sensible of any indisposition herself from this exposure, nor had she any appearance of the small-pox.

This case then decisively proves, that the small-pox virus may affect the human frame, even to its inmost recesses, although apparently secured from its effects, and yet give no evidence of its presence by exciting any perceptible disorder.

Another case, in its general character similar to the above, was lately communicated to me by Mr. Henry Gervis, a surgeon of eminence at Ashburton, in Devonshire. Mr. Gervis says, "The small-pox having appeared in the village of Woolson Green, about three miles from Ashburton, on the 6th of May, 1808, I vaccinated a poor woman, the wife of James Baskwell, who was in the last month of her pregnancy. Her three children had been inoculated the preceding day with variolous matter by the surgeon who attended the poor of the parish, and who had very properly declined inoculating her also from her particular situation. I made two punctures in each arm, each of which fortunately succeeded, and she regularly



passed the disorder, complaining only on the tenth and eleventh days, when the areola was most extended as is usual. I saw her very frequently during the progress of her disorder, and once or twice after its complete termination: I therefore can speak positively, that during that time she laboured under no symptom but what is connected with the cow-pox. From this period she continued perfectly well, and on Saturday last the 11th instant, she was delivered of a female child, having at the time of its birth many eruptions on it, bearing much the appearance of small-pox in the early stage of the disease. This event happened five weeks after her vaccination, and one month after she had been exposed to the variolous infection of her own three children, and that of several other persons in the same village. On the 14th I visited the child again, when I found the eruptions had increased to some thousands, perfectly distinct, and their character well marked. Many among the most respectable physicians and surgeons from Totness, Ashburton, and the neighbourhood, were kind enough, at my request, to come to the poor woman's place of abode, and witness the fact. But to put the matter beyond all doubt, I armed some lancets with the virus, and produced the small-pox by inoculating with it. On the 18th the infant was seized with slight convulsions, and on the morning of the 19th it expired.

"In addition to the circumstance of the mother's conveying the variolous infection to her unborn child, without feeling any indisposition from its action on her own constitution, I must remark that there cannot be a stronger proof of the efficacy of vaccine inoculation than this case affords. But happily proofs are not wanted, or I could give my testimony to a great extent."

Without producing more examples of a similar description at present, though many are before me, I shall conclude by observing, that a fact not unlike the preceding fell under the observation of Dr. Mead, who, in his discourse on the small-pox (chap. iv. page 337, edit. 1772) says, "A certain woman, who had formerly had the small-pox, and was now near her reckoning, attended her husband in the distemper. She went her full time, and was delivered of a dead child. It may be needless to observe that she did not catch it on this occasion; but the dead body of the infant was a horrid sight, being all



over covered with the pustules; a manifest sign that it died of the disease before it came into the world."

My principal object in the foregoing observations is, to guard those who may think fit to inoculate with variolous matter, after vaccination, from unnecessary alarms: a pustule may sometimes be thus excited, as on those who have previously gone through the small-pox; febrile action in the constitution may follow; and, as has been exemplified, a slight eruption.

At the commencement of vaccination I deemed this test of security necessary; but I now feel confident that we have one of equal efficacy, and infinitely less hazardous, in the re-insertion of the vaccine lymph.

November 18, 1808.

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### *A case of Aneurism of the Carotid Artery.*

BY ASTLEY COOPER, ESQ. F. R. S. SURGEON TO GUY'S HOSPITAL.

From the Medical and Chirurgical Transactions.

MARY EDWARDS, aged 44, was brought to my house by Mr. Robert Pugh, of Gracechurch-street, that I might examine a tumor in the neck, which was obviously an Aneurism of the right Carotid Artery. I advised her to become a patient in Guy's Hospital, and she was admitted on the 23d of October, 1805. The account she gave of the disease was, that the tumor appeared five months before, situated rather above the middle of the neck; its size at first being only that of the end of the finger; that it beat with very great force, and occasioned a strong pulsation in the brain; that it gradually increased upwards, until it reached the lower jaw, and extended downwards below the middle of the neck; that for a fortnight previous to her admission, the pulsation in it and the brain had been so strong as to prevent her sleeping; that the scalp on that side was tender, so as scarcely to bear the touch; that she had great difficulty in taking any solid food, and was constantly teased with a violent cough.

Upon examination of the swelling I found that it occupied two-thirds of the neck; it had a very strong pulsatory motion, and the skin was thin at its most prominent part. When the

swelling was examined at the hospital, great doubts were entertained if there was sufficient space between the clavicle and the tumor for the application of a ligature, and as her husband objected to the operation, she quitted the hospital.

In a few days, hearing that all her symptoms were increased, I called at her house, and strongly pointing out the probability of a fatal termination of the disease, I gained her consent and that of her relations to an operation.

On Friday, November 1st, 1805, the operation was performed in the presence of Mr. Pearce, surgeon, and Mr. Owen, apothecary to the Universal Dispensary, Ratcliffe Highway, of Mr. Travers, surgeon, and that of five other medical gentlemen.

The tumor at this time reached from near the chin beyond the angle of the jaw, and extended downward to within two and a half inches of the clavicle. I made an incision two inches long, on the inner edge of the sterno-mastoid muscle, from the lower part of the tumor to the clavicle, which laid bare the omo- and sterno-hyoideus muscles, which being drawn aside toward the trachea, exposed the jugular vein. The motion of this vein produced the only difficulty in the operation, as under the different states of breathing it sometimes presented itself to the knife, tense, and distended, and then as suddenly collapsed. Passing my finger into the wound to confine that vein, I made an incision upon the carotid artery, and having laid it bare, I separated it from the par vagum, and introduced a curved aneurismal needle under it, taking care to exclude the recurrent nerve on the one hand, and the par vagum on the other. The two threads were then tied about half an inch asunder, being the greatest distance to which they could be separated; I thought it proper not to run the risk of a hemorrhage by dividing the artery, as I was fearful the ligatures would be thrown off by the force of the heart, and the distance was too small to allow of any means being used to prevent it. As soon as the threads were tied, all pulsation in the tumor ceased, and the operation being concluded, and the wound superficially dressed, she rose from the chair in which she sat during the operation, and was immediately seized with a fit of coughing, which I thought would have terminated her existence. This seemed to arise from an accumulation of mucus in the trachea, which



she could not expel; it continued about half an hour when she became more tranquil.

*Saturday, Nov. 2.*—Mr. Owen, who had sat up with her, reported that she had slept six hours during the night, but was now and then disturbed by her cough. The pulsation in the tumor has not returned; that in the brain has ceased, and there is no appearance of diminution of nervous energy in any part of the body.

*Sunday, Nov. 3.*—Last night as she had some pain in her head, leeches were applied. To-day the pain in her head is gone; her cough is less troublesome; her stools and urine are natural; pulse 96.

*Monday, Nov. 4.*—Slept six hours last night; her spirits are good; pulse 100.

*Tuesday, Nov. 5.*—In the afternoon, I found her, as may be supposed contrary to my orders, sitting before the fire with three other persons, drinking tea which she swallowed with great difficulty; she had no pain in her head; her pulse 96, and the only circumstance of which she complains, is that her cough is troublesome.

*Wednesday, Nov. 6.*—In a violent fit of coughing last night, a slight discharge of venous blood took place from the wound. Mr. Hopkie, of Ratcliff-Highway, was called to her; but the bleeding ceased with the cough, and a piece of lint was laid lightly on the wound; in the afternoon her cough was less troublesome; her pulse only 92.

*Thursday, Nov. 7.*—My colleague, Mr. Forster, accompanied me to see her and to make a drawing of the tumor, which he thought was reduced one-third. She slept eight hours last night; her pulse 94.

*Friday, Nov. 8.*—Evening; I was sent for by Mr. Owen and Mr. Roberts, who alternately sat up with her, on account of their observing, that her left arm and leg were paralytic. I found them benumbed, and she moved them with great difficulty; but as her pulse was weak, and she laboured under considerable constitutional irritation, I thought the powers of these parts would be restored as her health improved. She had passed a very restless night, complaining that her bones were sore, and that her teeth felt as if softened. Her head is free from pain.

*Saturday, Nov. 9.*—Her cough is less troublesome; her pulse is 90; her spirits good; she talks with cheerfulness, and moves her arm with more facility than yesterday. She slept eight hours last night; she said she must have something to eat, but upon attempting to swallow solids she was incapable of doing so. She has no pain either in the head or tumor, but says, when she coughs she feels a pricking pain in the wound.

*Sunday, Nov. 10.*—I did not see her.

*Monday, Nov. 11.*—She had passed a good night; her left arm she now moves with more facility, but I thought with not quite so much ease as the other. She is in good spirits, and has some appetite, but cannot swallow solids. Her chief sustenance is arrow root, to which, as she had been very much accustomed to take spirits, a little wine is added.—Her cough is sometimes very violent; her pulse is only 84; the ligatures are projecting further from the wound, than at any time since the operation.

*Tuesday, Nov. 12.*—My colleague, Mr. Lucas, accompanied me to see the woman, this day. We found her in good spirits, and the pulse only 82; her cough less troublesome, and she was able to sit up and use her arm with so much facility that it required that the attention should be particularly directed to the part, to discover any difference in the powers of the two arms.

When the dressings were removed, the ligatures were drawn from the wound, including the intervening portion of artery. The edges of the wound were then brought together by adhesive plaster.

*Wednesday, Nov. 13.*—Her cough is less troublesome; she swallows liquids with more ease. The only complaint she makes is of a pain in the back, of which she was relieved by a dose of *magnesia vitriolata*.

*Thursday, Nov. 14.*—She slept eight hours last night, and her state is in every respect improved; she swallows with less difficulty; the tumor is reducing in size, and is entirely untended with pain. As I now considered her out of danger, I did not visit her on Friday or Saturday; but Mr. Jones, one of my house-pupils, visited her and found the wound nearly closed.



*Sunday, Nov. 17.*—I was much disappointed to find her labouring under a high degree of constitutional irritation; the tumor was also increased and very sore upon pressure; the wound was as large as immediately after the operation, and discharged a sanious serum; she complained of a great difficulty in swallowing, and of a most distressing cough after the fits of which she hooped violently; her pulse 96; and her left arm again weaker than the other.

*Monday, Nov. 18.*—She had passed a restless night; complaints of pain in her head, and the size of the tumor has increased; there is great soreness upon the neck, when it is pressed; the pulse is quick, and the tongue is furred.

*Tuesday, Nov. 19.*—Her pulse is very quick; she had no sleep last night, although she took forty drops of tincture of opium; the tumor is still more increased, and the skin over it of a brownish red colour.

*Tuesday, Nov. 20.*—She had slept three hours last night; her pulse is 108, and small; she is unable to swallow even her saliva, which constantly dribbles from her mouth, and every attempt at deglutition, produces a violent cough.

*Evening.*—Her pulse 120; she is in a profuse sweat; and still unable to swallow.

*Thursday, Nov. 21.*—She died.

### DISSECTION.

The aneurismal sac was found inflamed, and around the clot of blood which it contained, there was a considerable quantity of pus.

The inflammation extended on the outside of the sac along the par vagum, nearly to the basis of the skull.

The glottis was almost closed, and the internal surface of the trachea was inflamed, coagulating lymph adhering to its mucous membrane.

The sudden increase which the parts had undergone from inflammation, added to the size of the tumor previous to the operation, had occasioned so much pressure upon the pharynx, that it would not easily admit a bougie of the size of a goose quill.

The nerves, as may be seen, sustained no injury, the ligature having passed between the recurrent and the artery on the one hand, and the par vagum on the other.

The cause of her death then, was the inflammation of the aneurismal sac and the parts adjacent, by which the size of the tumor became increased so as to press on the pharynx and prevent deglutition, and upon the larynx, so as to excite violent fits of coughing, and ultimately impede respiration.

A similar event, however, may be in future prevented, by performing the operation when the tumor is small, and pressure has not been made by it upon important parts, or if it is of considerable size, as in this case, by opening the tumor and discharging the coagulum, as soon as inflammation appears.

As I could not obtain permission to open the head, the cause of the paralysis remains unknown. It did not immediately succeed the operation, but was observed first on the eighth day after it. It came on whilst she laboured under great constitutional irritation, lessened as it decreased, and returned when the irritation became greater; but as it appeared that the irritation which she suffered was owing to the operation being too long deferred, it will not prevent my performing it in any case in which the disease is somewhat less advanced.

It appears that no objection can be made to this operation on account of any unusual danger of bleeding at the time the ligatures separate, since, although they were discharged from the wound on the twelfth day, and they were certainly separated from the artery on the eleventh, the ulcerated extremity of the vessel had been closed by the adhesive process and by a clot of blood which adhered strongly to its coats. Hence we may conclude, therefore, that the carotid artery may be, in this respect, as safely tied as any other artery in the body.

January 29, 1806.



*Second case of Carotid Aneurism.*

BY ASTLEY COOPER, ESQ. F. R. S. SURGEON TO GUY'S HOSPITAL.

From the Medical and Chirurgical Transactions.

IN the year 1806, I had the honour of presenting to this Society an account of an operation for carotid aneurism, which terminated unsuccessfully.

I then took the liberty of observing, that I thought that the result of that case ought to have no influence in preventing a similar operation under more favourable circumstances, as the death of the patient was attributable to the advanced stage of the disease at the time of the operation. The aneurismal tumor had become so large, as to press upon the air tube and occasion cough, difficulty of breathing, and even effusion of coagulable lymph upon its internal surface; and it had also compressed the pharynx, in a degree to prevent even the passage of fluids into the stomach.

I had no apprehension of the functions of the brain sustaining any permanent injury from a ligature on the carotid artery, having the evidence of Dr. Baillie to prove, that one carotid had been entirely obstructed, and the diameter of the other considerably lessened in the same person, without any apparent ill effects. I have also given a drawing in my former paper, of the left carotid artery being obstructed by the pressure of an aneurism of the aorta; and we have the analogy of animals to show, that both carotids may be tied without any visible effect upon the functions of the nervous system. I had many years ago made the experiment of tying these vessels in the dog, and immediately as it was concluded the animal was placed upon the ground, and showed no diminution of voluntary powers. It exhibited the same fears and affections as before, and betrayed no greater loss of appetite, or disposition to sleep than an animal usually does who has been the subject of an experiment which gives some pain, but does not injure the vital functions. A preparation made from this dog has been long preserved in the collection at St. Thomas's Hospital; and the parts have been injected to show the principal vessels which carry on the circulation, which are the two thyroideal arteries, and two

branches from each vertebral, passing under the angles of the lower jaw.

I was therefore resolved to take the first opportunity of repeating the operation before the disease had advanced so far as to interrupt, in a considerable degree, any other function than that of the passage of blood through the carotid artery; and my friend Mr. George Young, surgeon, had the kindness to furnish me with a case of the most favourable nature, by sending, first to my house, and afterwards to Guy's Hospital, a man who was in all respects the subject I should have chosen. He had a mind cool and firm, yet obedient to every injunction: a body equally unirritable as his mind—the tumor only about two inches and a half in diameter, and the neck of considerable length, so as to give ample space for exposing the carotid artery. The history of this case is as follows:—

HUMPHREY HUMPHREYS, aged fifty, who has been employed to carry loads of iron\* as a porter, observed six or seven months ago, a tumor, having a pulsatory motion, and about the size of a walnut, on the left side of the neck, just under the angle of the jaw, and extending from thence downwards to the thyroid cartilage. It was accompanied with great pain on the left side of the head, which began about five months ago, and was attended with a sense of pulsatory motion in the brain. The tumor affected his speech, so as to make him extremely hoarse; and he had more recently a cough attended with slight difficulty of breathing, and which seemed to be the effect of the pressure of the swelling on the larynx. His appetite was sometimes affected by it; for three or four days he eat heartily, and then for many lost his relish for food. He had a sense of coldness succeeded by heat in his left ear, and he often became sick when eating, but did not vomit. Upon attempting to stoop at any time from that period, he had an insupportable feeling as if his head would burst; a giddiness; loss of sight; and almost total insensibility.

The left eye, which had for some time been gradually closing, appeared now not above half as large as the right; yet its power of vision was equally perfect.

\* The employment consists in this:—A collar of wood is placed around the neck and upon the shoulders, and he carries bars of iron on each shoulder, thus protected.



A blister was at this time ordered to be applied on the head by Dr. Hamilton, which lessened his pain. A month ago he applied another with the same relief; but it lasted only for a few days. He continued at work until the day previous to the operation.

The dilatation of the carotid artery was seated just below the angle of the jaw, and about the acute angle which is made by the great division of the common carotid. The tumor was about the size of a pullet's egg, and prominent in its middle.

The pulsation of the aneurism on the day of the operation was remarkably strong; when the sac was emptied by pressure on the artery below, the tumor sprang to its original size with one contraction of the heart.

I proposed to tie the common carotid below the dilated part, and the operation was performed at one o'clock on the twenty-second of June, 1808, at Guy's Hospital.

I began my incision opposite the middle of the thyroid cartilage from the base of the tumor, and extended it to within an inch of the clavicle, on the inner side of the mastoid muscle. On raising the margin of this muscle, the omo-hyoideus could be distinctly seen crossing the sheath of the vessels, and the nervus descendens noni was also exposed. I next separated the mastoid from the omo-hyoideus muscle, and the jugular vein became apparent, which being distended at every expiration spread itself over the artery. Drawing aside the vein, the par vagum was evident, lying between it and the carotid artery, but a little to its outer side. This nerve was easily avoided.

A blunt iron probe constructed for the purpose was then passed under the artery, carrying a double ligature with it. Two ligatures being thus conveyed under the artery the lower was immediately tied. I next detached the artery from the surrounding parts, to the extent of an inch above the lower ligature, and then tied the upper. Lastly, a needle and thread were passed through the artery above one ligature and below the other. The division of the artery was then performed.

Nothing now remained but to dress the patient, and this was done by drawing the parts together, by adhesive straps, the ligatures hanging from each end of the wound, and by laying on a piece of lint retained by straps of adhesive plaster.

Mr. Vose, my dresser, (whose attention to the case was unremitted, and to whose care and knowledge many of my patients have been indebted for their recovery) now asked the patient if he experienced any unusual sensations about his head. He answered, that for the first time since two months after the formation of the tumor, he was relieved from a distressing pain which extended up the left temple, accompanied by a violent throbbing of all the arteries of that side. This pain never returned.

The pulsation in the tumor, however, had not entirely ceased, although it was so much diminished as to become obscure; but it was felt by my colleague, Mr. Forster, by Mr. George Young, Mr. Dubois, jr. from Paris, who accompanied Mr. Young, by Dr. de Souza, and many others who were present at the operation. I concluded it to be the effect of the return of blood by the internal carotid artery from the brain, in consequence of the free anastomosis which exists between the blood vessels within the skull.

The patient was put to bed, with his head elevated, and in this position he felt quite comfortable.

*Three p. m.* Pulse was moderate, skin cool, suffered very little pain. Pulsation in the tumor perceptible, but inconsiderable, when contrasted with its force before the vessel was tied.

*Five p. m.* Pulse stronger and fuller, but in other respects as before; head entirely free from pain.

*Eight p. m.* Patient's pulse reduced to the healthy standard, skin cool; says he feels no pain.

*June 23.—Six a. m.* Patient passed a good night.

*One p. m.* I saw the patient; he had a slight cough; has had no evacuation since the operation; pulse was not quicker than natural.

*Ten p. m.* The patient got out of bed, and went to the water-closet, and had an evacuation.

*June 24.—Six a. m.* Pulse natural; pulsation in the tumor continues; tumor sore when compressed; has become firm, for the blood which was fluid in it prior to the operation and all yesterday is now coagulated; pain, and a sense of fulness felt on the right side of the head.

*June 25.—Six a. m.* Patient says he no longer feels pain in any part; has had a good night; has only one troublesome



symptom, viz. an occasional rattling in the larynx from accumulated mucus; pulse this morning quite temperate.

*Three p. m.* The tumor is considerably diminished; pulse moderate; no constitutional irritation.

*June 26.—Eight a. m.* Patient had a good night; pulse still moderate; skin cool.

*Eleven p. m.* Still free from any disagreeable symptom.

*June 27.—Seven a. m.* Patient very restless during the night; coughed much and had pain in the head; spirits depressed; pulse natural.

*Half past one p. m.* Pulse eighty-four; feels much better than in the morning; has had an evacuation from the bowels since last night.

*June 28.—Seven a. m.* Pulse natural; had a tolerable night; bowels open; no pain.

*One p. m.* I saw the patient; pulse eighty-four; slight pulsation still to be felt in the tumor, which is much diminished.

*June 29.* Pulse natural; no pain; pulsation still perceptible; tumor so much less that the skin is wrinkled over it.

*June 30.* Wound dressed the first time, and has united by the first intention as far as the ligatures would permit; he is free from irritation.

*July 1.* Pulse natural; man tranquil; pulsation very obscure; tumor firm; he is very hoarse.

*July 2.* No stool; ordered opening medicine; very hoarse, so as to speak only in a loud whisper.

*July 3.* Pulsation doubtful; man healthy.

*July 4.* Going on well.

*July 5.* Wound looks well; man appears natural; but the hoarseness continues.

*July 6.* He is free from any symptoms of irritation.

*July 8.* Patient says the tumor is now only half its size at the time of the operation.

*July 9.* Ordered a poultice.

*July 12.* Ligatures projecting more; and much more discharge from the wound.

*July 14.* Upper ligature came away, being removed by Mr. Vose.

*July 15.* Lower ligature came away; pulsation very obscure.

*July 17.* Man walked out of his ward; the tumor at this period was reduced to less than half its size. The pulsation in it was with difficulty perceived; but it continued until the beginning of September, at which period all who saw him agreed that the pulsation had ceased, and the tumor was then scarcely apparent. The facial and temporal arteries on the left side cannot be felt.

The wound was a long time in healing, first from a sinus in the course of the ligatures, and afterwards from a fungus where the sinus had been placed.

The man was discharged, cured, on the 14th day of September, and returned to the occupation of a porter at Crawshay's iron wharf, Thames-street.

The result of this case afforded me a degree of pleasure which compensated for the disappointment I felt in the issue of the former. In a professional point of view, it was highly desirable to ascertain the possibility of saving life in a case which had hitherto proved generally fatal; and I could not but feel more than common interest in the fate of a man, who, although he well knew that the trial was new, and the risk considerable, never betrayed the smallest signs of apprehension.

Near eight months have now elapsed since the operation was performed, and he has returned to his former employment without any diminution of his mental or corporeal powers, excepting the lessened action of the temporal and facial arteries on the side in which he was operated. The tumor has disappeared, and he has not been since subject to that pain in the head, by which he had been so much distressed prior to the operation.

This aneurism, from the depth of its situation, was, I believe, seated in the internal carotid artery, and this led me to hope that the regurgitation of the blood, although at first sufficient to produce a slight pulsation in the tumor, would not continue to support its growth, because as the internal carotid passes through a foramen in the skull, a little above the swelling, it could not dilate at that part to bring down any additional quantity of blood into the sac; so that its first effect was likely to be as great as any it could produce. But if the aneurism had been of the external carotid artery, owing to the number of communicating vessels, I should not have been equally san-



guine in my expectation that the pulsation would have ceased, as I have known two instances, one of a wounded radial artery, and the other of aneurism of the anterior tibial, in which the tumor continued to grow by anastomosis, after the arteries had been tied above the swellings.

February 21, 1809.

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*A case of Exposure to the Vapour of Burning Charcoal.*

BY WILLIAM BABINGTON, M. D. F. R. S.

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From the Medical and Chirurgical Transactions.

A CONSIDERABLE degree of interest having been excited by the melancholy accident which occurred at the Dolphin, a public house, in Honey-lane Market, on the morning of the 24th of November last, I am induced to offer to the consideration of the society the following statement with some reflections on the subject of it.

William Smith, aged thirty-eight, a waiter belonging to the house, and a boy about thirteen years old had, on the preceding night, gone to their bed-room, both in perfect health, and it was supposed under ordinary circumstances. Between six and seven o'clock in the morning, neither of them having been then seen, a person went to the chamber, and on opening the door found the waiter insensible, and apparently at the point of death; the poor lad lying lifeless on the floor, and a chafing-dish containing some extinguished charcoal placed at the foot of the bedstead. The one who still exhibited signs of life was removed into a larger and more airy apartment; and Mr. Hingeston, of Cheapside, being called, and finding that with stertorous breathing the countenance was rather flushed, the lips livid, the pulse full and strong, and as he conjectured, about ninety in the minute, was induced to take away eight, or at most ten ounces of blood from a vein in the left arm. At his request I attended to give what assistance might lie in my power. It was little more than seven o'clock when I reached the house, and having been informed of the circumstances under which these unfortunate persons had been discovered, as

the respiration, pulse, and heat of him who had been removed into a cool and pure air were distinctly perceptible, I thought it my duty to determine, by actual experiment, whether any thing could be done for the recovery of his companion. With this object in view, a common sized silver catheter being passed from the mouth into the trachea, an artificial process of respiration was instituted by the aid of my friend Mr. Hingeston, and that of his assistant Mr. Gingell, alternately breathing into the lungs, and forcing out the air so introduced by means of external pressure.

While these gentlemen were engaged in this process, I went to my friend Mr. Allen, in Plough Court, to request his co-operation in the trial of the galvanic influence, as one of the most ready and satisfactory modes of ascertaining whether any remains of life still existed to encourage our further exertions. In this interval the waiter, whom I had visited as I was quitting the house, had evidently become worse, and the artificial respiration having produced no effect on the boy, we passed several shocks from the galvanic trough, which we had previously found to be acting with considerable power, through his chest and head; but these being equally unproductive of any visible excitement we were reluctantly compelled to think that he had in reality passed from sleep to death. As we now perceived that the vital powers were in Smith still further enfeebled, and that unless more active steps were taken, there was reason to think that he also would fall a victim to the accident, we transferred the galvanic apparatus to his apartment, the window of which being open, and there being no fire, the temperature was necessarily that of the atmosphere, about 50 degrees of Fahrenheit. At this time his pulse, instead of being strong and full, as before the bleeding, was weak and quick, and the respirations very imperfect; but the heat of the upper part of the body remained. The power of voluntary motion was suspended, and the slight convulsive twitchings of the muscles which at first prevailed had altogether ceased; ordinary impressions no longer appeared to affect his organs of sense. His countenance was pale, the eyelids closed, the eyes prominent and rolling in their sockets from side to side; the tongue swollen and projected from the mouth, was locked in this position by a spasmodic action of the muscles which raise the lower jaw. At



the corners of the mouth there was a copious discharge of frothy saliva. Having passed a galvanic shock through the chest, he instantly to our surprise, drew his breath deep. The muscles of the abdomen were seen to react, though feebly, while those of the face were slightly convulsed, and the eyelids were raised. At each successive application of this powerful agent the respirations were more forcibly performed, and the stroke of the artery at the wrist rose in the same proportion. Having, between eight and nine o'clock, procured a bladder filled with oxygen gas, we caused it to be inspired, and we thought that it was followed by an increased activity of the powers of respiration and circulation. As the heat of the body was not deficient, we now sprinkled the face and chest with cold water, which also had the effect of rousing the dormant powers of sensation, as the respiratory muscles were uniformly thrown by it into action, though in a more feeble and interrupted manner than when we employed the galvanic influence. About nine o'clock, having received a large supply of oxygen gas, we repeated the inhalation and the galvanic succussions alternately, through the chest and head, every half hour, till twelve o'clock, when the galvanic application was discontinued, as the heart, though uniformly excited by it, seemed in the intervals to act more feebly, and we were apprehensive that by exalting the action of one power continually, we might destroy that equilibrium of forces which is necessary to the maintenance of life. Some volatile spirit of hartshorn was rubbed upon the temples and chest, and the vapour of it inhaled; the latter rendered him uneasy and excited coughing. As the extremities were cold, bottles filled with hot water were applied to his feet, and the trunk of the body covered with the bed clothes. About one the surface became moist, and gradually a warm perspiration was diffused over the whole body. The pulse from this time was uniformly fuller and more equable in strength and frequency, and as the respirations became more free, the inhalation of the oxygen was repeated at longer intervals, and about four in the afternoon was entirely desisted from. The spasm of the jaw having subsided in a great degree, and the tongue being reduced, he was allowed some very weak wine and water, which he swallowed, a tea spoonful at a time, without any apparent difficulty. The pulse had now risen to 120, and was

strong; we, therefore, in the early part of the evening, considered of the propriety of further venesection; but as the apoplectic stertor was evidently not so great, we determined not to interfere with the efforts of nature; accident, however, accomplished what we had hesitated to do, the ligature having slipped from his arm, he lost, before it was noticed, nearly a pound of blood; the pulse sunk, his countenance collapsed, and Mr. Hingeston, who saw him at this period, was apprehensive that he would not survive the debilitating influence of this accidental hemorrhage. At twelve at night he had, however, recovered from this depression; the pulse was 120, full, but soft. Repeated glysters had procured a copious stool, and fomentations of the belly had been followed by a free discharge of urine. He was now very restless, and Mr. Gingell kindly stayed with a relation of the poor man during the night, when the exertions of both were requisite to keep him from injuring himself, and rolling out of bed. On the 25th, he recovered the use of speech, though very imperfectly. The tongue, when put out, pointed to the left, and the muscles on that side of the face were slightly paralysed, as they had been occasionally observed to be on the preceding day; he complained, when questioned, of pain in his left side; referred to the spot where the wire from the galvanic trough had been applied, and also in his left arm and hand, the latter of which was swelled; the orifice in the arm did not, however, exhibit any signs of inflammation. The cough, which had occasionally distressed him the preceding day, was still rather troublesome. His mental powers seemed much impaired, and he cried bitterly when any person entered the room, a state of imbecility frequently observed in cases of ordinary palsy. He did not recollect one event, or one sensation from the time when on lying down between the hours of eleven and twelve, he saw the charcoal burning bright, till the second or third day after the accident, when he became conscious that he was in a strange room, and felt great distress from being in a situation of which he could form no adequate and satisfactory conception. As the tongue was white, the temperature rather augmented to the touch, and the pulse full, he was kept on a low diet, and the bowels regularly purged. He progressively recovered so far as to be removed by his relations in the course of a few days.



On December 15, he called at my house; at which time he only complained of a slight degree of weakness, more particularly felt in his left arm. His appetite was good. No affection of his head, nor febrile symptoms.

W. BABINGTON.

January 7, 1807.

### REFLECTIONS.

THE vapour of burning charcoal has, from times of ancient date, been observed to exert a very deleterious influence on the life of animals; but it is only within these few years that its nature has been developed, and its mode of operating fully appreciated. Modern chemistry has clearly ascertained the composition of atmospheric air, and proved that its capacity for supporting life chiefly depends upon that constituent principle which, in the language of this science, has been denominated oxygen. It has likewise instructed us that there are many other aerial fluids which possess the ordinary physical properties of the atmosphere, but differ essentially from it in not being subservient to the purposes of respiration. They have hence been distinguished from it by the general term of irrespirable airs or gases, and are destructive of life either by the exclusion of what is necessary, or by the presence of principles directly injurious to vital energy. Even the atmosphere that we breathe may, by this very process, as well as by various circumstances of chemical combination, be either itself deteriorated, or it may be impregnated with other fluids, and thus be rendered incapable of supporting the function of respiration. We find that the burning of bodies is one of the most common, but effectual ways of producing this change, which analysis teaches us it does by the abstraction or condensation of the oxygenous portion of atmospheric air, and the evolution of new products corresponding with the nature of the inflammable substance which is made the subject of the experiment.

When charcoal in combustion combines with oxygen we obtain carbonic acid gas, and at the same time in proportion to its moisture, more or less hydrocarbonous gas is evolved. The latter is peculiarly fatal to life, and very much increases the danger resulting from exposure to the vapour of burning fuel. The only attempt ever made to breathe it in its undiluted state

with which we are acquainted, was that by Mr. Davy, who was very nearly killed by three inspirations. Even when mingled with atmospheric air it very powerfully depresses the living energy. The carbonic acid gas, when unmixed, is also very quickly destructive, and from its specific gravity being considerably greater than that of the atmosphere, it is disposed to accumulate in particular situations, and has in such proved a frequent source of death. Rozier and Davy conclude from their experiments, that the carbonic acid in its undiluted form is wholly irrespirable from its exciting a spasmodic action in which the epiglottis is closed, and the entrance of this fluid into the lungs altogether prevented. The effect produced by it therefore is similar to what occurs in the act of drowning; but if this be the case, and the carbonic acid exert no deleterious influence on the nervous and vascular systems, how shall we explain the fact, that the loss of irritability in the muscles of animals which have been destroyed by immersion in noxious airs is comparatively greater than in such as are hanged or drowned? The fatality of this gas to the different classes of animals is in general in proportion to the more complicated system of their organization. On the human constitution according to the period of exposure and the degree of concentration, its effects vary through all the shades of giddiness, pain of the head, anxiety, depression of the powers of life, impeded respiration, loss of voluntary motion and sensation, to the complete suspension of the functions of respiration and circulation. That this pause in the series of vital phenomena shall be final, it is not necessary that any change in the structure of the body shall be observable; we have, however, no right to conclude it such, though appearances should be unfavourable, until our exertions shall have been employed in vain, or that order of combinations which tends to destroy the fabric of organized bodies shall have already commenced. We know not what life is, nor can we without the imputation of temerity speculate on the nature of that mysterious principle which regulates the functions of living beings. While, therefore, there is no apparent læsion of the system, we are called upon to rouse, if possible, its latent energies, by restoring the action of those parts which are more immediately subservient to life; and experience teaches us, that by inflating the lungs we change the condition of the



blood, and that there exists a chain of connexion between the circulation of blood that has undergone the respiratory process, and the maintenance of that state of the organs of sense and motion which fits them for discharging their respective offices.

Before we proceed to consider the plan followed on the occasion which is the more immediate subject of this communication, as the inquiry involves some points of important practical moment, let us endeavour to ascertain what are the appearances which have been remarked in the bodies of animals killed by the vapour of burning fuel. We were refused permission to examine the body of the boy who died; but the researches of others inform us, that there is, in animals furnished with a double heart, a deficiency, either positive or relative, in the quantity of blood contained in the left side of the heart, and in that series of vessels which is connected with it, and an accumulation in the right side of this organ and in its dependent system; and that this condition extends to the veins of the head. In some instances also the ventricles of the brain, it is said, have been observed to contain a frothy serum, occasionally tinged with blood. The lungs have been found collapsed with more or less of frothy liquid in the bronchiæ, and the viscera of the abdomen turgid with blood. Such are the principal anatomical facts: how far do they point out an active and successful mode of proceeding?

The first, and most natural step to be taken, consists in the exposure of the person to a free and pure air, and if the respiration be impeded or suspended we should endeavour to restore this function as speedily as we can, both with a view to the chemical changes to which we have alluded, and to the mechanical effect which the inflation of the lungs has in promoting the circulation of the blood through the minute branches of the pulmonary vessels. It has been insisted upon by very respectable authority, that the abstraction of a small quantity of blood should be the measure first adopted, in order that the heart and the large vessels may, by being relieved from their overdistended state, be enabled to contract, and that the brain also may be freed from pressure. But as there is, previously to the accident, no disposition to this accumulation, and as the condition of the lungs affords a mechanical obstacle to the transmission of the blood, we deem it more advisable to remove this by

causing them to expand, and if the heart should not then freely act, or the symptoms of pressure should continue equally urgent, it will be full time to have recourse to the lancet. In our patient the pulse certainly sunk after the bleeding, if not owing to this operation, and the accidental hemorrhage which occurred in the evening, it was feared by the attendants would have proved fatal to him. In what way, may it also be asked, shall we get rid of that accumulation which occurs in the vessels of the liver and other important organs which occupy the abdominal cavity? The restoration of respiration seems to be the natural and most effectual remedy to the altered balance in the circulating system in general. If, however, the loss of blood be thought necessary, there can be little doubt that opening the jugular vein will most quickly answer the end proposed.

The inhalation of oxygen gas was directed under the idea of there being an accumulation of that carbonaceous matter which, under the ordinary circumstances of respiration is regularly thrown off; and on account of the evident depression of the vital actions produced by the noxious effects of the carbonic and hydrocarbonous gases. As far as we could judge from the state of the pulse it seemed to exert a stimulating influence on the arterial system at least. The propriety of exciting the heart by electricity previous to the establishment of the respiratory process has been questioned by many. On the present occasion we employed the galvanic modification of this principle, and its effect in promoting a deep and complete inspiration, followed by a more vigorous action of the heart and arteries was strongly conspicuous. As the galvanic apparatus is very portable, and admits of easy application, and as the evolution of this principle is not affected by the state of the atmosphere, it seems to be preferable in cases of this kind to the ordinary form of electricity. By diminishing or increasing the number of compartments when we make use of a trough, we can completely regulate the quantity of the power that we employ.

The introduction of fluids into the stomach is not an easy process in many cases of suspended animation, as trismus is by no means an uncommon occurrence. In such instances, however, this is a circumstance of secondary importance.



Portal, and other French writers, strongly recommend the employment of acids.

In Russia where accidents of this description are very common, it is the general practice to rub the body with snow, and it is said with the happiest effect. What may be the state of the animal temperature in this affection is at present merely a matter of conjecture. The skin is for the most part warm, but as yet we have no thermometric observations. The Russian plan is probably of use, from the strong impression which is made upon the skin as a sentient organ. The sprinkling of cold water upon the face and breast is a less energetic application of the same principle, but has considerable effect in rousing persons both from this state and that of syncope. On the present occasion it constantly produced a visible and marked contraction of the respiratory muscles, though inferior in degree to that resulting from the galvanic stimulus. It is also a well known fact, that the recovery of the dogs which are made the subjects of experiment in the Grotto del Cani, is much favoured by their being plunged into a neighbouring lake.

In some cases warmth may be required to be employed, and clysters form an easy mode of effecting this, and at the same time of administering stimulants.

The application of bottles filled with hot water to the feet was the mode followed with Smith, when the apparent depression of the vital powers seemed to demand the aid of artificial heat, and it probably contributed to induce the general and copious perspiration which shortly afterwards broke out.

The torpor of the bowels and urinary organs in Smith were but a part of that general inactivity which arose from the obstructed action of the vital functions, and the directly poisonous influence of the vapour.

It is of advantage in such cases, to apply volatile alkali, or other pungent bodies to the inside of the nostrils. Whatever promotes sneezing or coughing gives a succussion to the diaphragm, and its antagonist muscles, and tends to reestablish the process of respiration.

Some care will be requisite in the after treatment to guard against any morbid increase of action. The subsequent febrile condition which occurred in our patient was neither severe nor of long continuance.



Whatever plan we adopt, our measures must be promptly and steadily carried into execution; and we may expect that our perseverance will occasionally be put to the test by the interference of some officious person. We were under the necessity of using threats to one of Smith's relations, who would willingly have consigned him to an untimely grave to free him from the misery and pain which she conceived we were cruelly inflicting. Happily, however, our endeavours to restore him succeeded. The exertions made to recover the poor boy, probably proved ineffectual, from the actions of life having been irrevocably suspended by the situation into which he was thrown. In falling out of bed, he was immediately exposed to the concentrated stratum of noxious air which occupied the lower part of the room.

It is to be regretted, that frequently as such accidents have occurred, the danger of sleeping in close rooms in which fires have been kindled, is still not sufficiently understood. From the testimony of the survivor on this melancholy occasion, it does not appear that he or his unfortunate companion were at all aware of the very perilous circumstances in which they placed themselves.

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*Observations on the Different Methods recommended for  
Detecting minute portions of Arsenic.*

BY JOHN BOSTOCK, M. D.

Read before the Liverpool Medical Society.

THE great importance of being able to detect small quantities of arsenic, in cases where there is reason to suspect that it has been employed with the intention of destroying life, is too obvious to be insisted on. We accordingly find, in most systems of chemistry, that directions are given for the application of those tests which afford the most decisive indications of its presence; and it is generally admitted, that these directions are of such a kind, that where the substance to be examined can be procured in sufficient quantity, and where the operator is sufficiently skilful in his manipulations, the existence of arsenic may be satisfactorily established. It, however,

frequently happens, that the circumstances mentioned above do not exist, but that the substance to be examined is only a minute residue of what had been employed; or that it is to be searched for among the contents of the stomach; or that the person to whose lot it falls to undertake the examination is previously inexperienced in chemical experiments, and unacquainted with the appearances which will be produced by his processes. These considerations have induced me to lay before the society the result of some observations that I have made on the subject. I propose to describe the processes so minutely, that a person who had never before seen them performed, or who was not in the habit of experimenting, might have some idea of the appearances which would be presented to him; and I shall point out what quantities of arsenic we may expect to be able to discover by the application of the different tests.

The latest publications which contain an account of the methods for detecting arsenic, are, Mr. Murray's system of chemistry, and the dictionary of chemistry by the Messrs. Aikins. From these valuable works we learn, that there are five processes that may be employed for this purpose; the precipitation of the arsenic, from any fluid in which it is dissolved, by an alkaline hydro-sulphuret; its precipitation by the sulphate of copper; the reduction of the oxyde into the metallic state, by heating it with extraneous substances in a glass tube; the effect which arsenic has in whitening copper, when heated in contact with it; and the peculiar odour which it exhales when evaporated from a heated surface. I shall make a few observations upon each of these processes, bearing in mind the object which induced me to enter upon the investigation, which is not to give information to the scientific chemist, but to assist those who may be under the necessity of stating an opinion upon subjects of this nature, although not in the habit of performing chemical experiments.

When only a minute quantity of an alkaline hydro-sulphuret is added to a solution of the white oxyde of arsenic, a precipitate is instantly formed of a bright orange colour. One drachm of water, in which ten drops of a strong solution of the sulphuret of potash had been diffused, had four drops of a saturated arsenical solution added; immediately an orange-coloured cream formed itself at the surface of the fluid, which, *in the course of twenty-four hours*, had nearly subsided,



forming a precipitate of the same colour at the bottom of the glass, and leaving the sides of the vessel slightly coated with a similar substance; the fluid was left transparent. The alkaline hydro-sulphurets are also precipitated by acids, and by other metallic salts, with results more or less resembling that produced by arsenic. To a similar solution of sulphureted hydrogen, one drop of sulphuric acid was added; the whole assumed a light milky appearance, and, after some time, a delicate yellow precipitate rose to the surface of the fluid, and coated the sides of the glass. The same experiment was made with a saturated solution of tartarized antimony; it required a larger quantity of the solution to produce the same effect; but, by the addition of ten drops, the appearances that presented themselves were very similar to those where the arsenical solution had been employed, the precipitate rising to the surface, and assuming a deep orange hue. The same experiment was performed with the nitrate of mercury, but the result was here considerably different; patches of a reddish precipitate were produced, which sunk irregularly; then the whole fluid became opaque, and of a dirty brown colour, which was gradually converted to a dull gray. In twenty-four hours there was a copious gray precipitate at the bottom of the glass, while the fluid was left transparent. With the oxygenated muriate of mercury, an irregular copious precipitate was produced, of a dull olive colour, which, after some time, became black, leaving the fluid of a dirty yellow. With the muriate of tin, a copious, dense, brick-coloured precipitate was instantly thrown down, which, in twenty-four hours, was converted into a muddy brown, leaving the fluid transparent. With the nitro-muriate of platina, a copious, clay-coloured precipitate was produced, which, after some time, subsided, and left the fluid transparent, and of a bright reddish-brown hue. Among these precipitates, which are all that are described as being in any degree similar to the hydro-sulphuret of arsenic, the greatest part are so obviously different, as not to afford any probability of mistake: the only two which bear a close resemblance to it are, the hydro-sulphuret of antimony, and the precipitate of sulphur, which is separated from the fluid by the addition of an acid. In this latter case, however, a sufficiently clear distinction is afforded by the colour of the substances,

the sulphur preserving its peculiar light yellow, while the arsenic presents a deep shade of orange. The difficulty of discrimination will only rest between the hydro-sulphurets of antimony and of arsenic; both the colour of the precipitate, and the mode of its formation, are so similar, that when the comparative experiment was made upon the two substances, in contiguous glasses, it was difficult to discover any visible mark of distinction. The arsenic is indeed more easily affected by this process, but this being a difference in the *degree* of the effect, is of no use where we are examining the nature of an unknown compound. Combined with the other processes for detecting arsenic, the effect of sulphurated hydrogen may be of some value, although it seems that it is not sufficiently discriminative to be depended upon alone. It discovers the presence of arsenic with great delicacy; sixty grains of water, to which one grain only of the liquid sulphuret had been added, was almost instantly rendered completely opake by 1-80th of a grain of the white oxyde of arsenic in solution.

The next test of arsenic on which I propose to remark, is the one that was discovered by Scheele; the beautiful green precipitate that is produced by the addition of the sulphate of copper to a mixed solution of the white oxyde of arsenic and potash. The green precipitate formed by this process so decidedly indicates the presence of arsenic, that I thought it desirable to ascertain, as exactly as possible, what were the best proportions in which the ingredients should be employed, and in what way they should be mixed, so as to exhibit the effect in the most obvious manner. After a number of trials, in which the substances were employed in various quantities, and under different circumstances, I am disposed to recommend that the proportions of the oxyde of arsenic, the potash, and the sulphate of copper, should be to each other as the numbers one, three, and five, respectively. For instance, if one grain of arsenic, and three grains of potash, be dissolved in two drachms of water, and, in another equal quantity of water five grains of the sulphate be dissolved, we have two solutions, which are transparent and nearly colourless; but, upon mixing them together, the whole is converted into the most beautiful grass green, from which a copious precipitate of the same hue slowly subsides, leaving the supernatant fluid



nearly without colour. If the same materials are employed in the same manner, but without the arsenic, a delicate sky-blue is formed, which is so decidedly different from the former colour, as not to admit of the possibility of error. The proportion of potash and of arsenic which I have recommended, although different from that pointed out by Scheele, is the one directed by Mr. Murray, and certainly that which I have found to afford the most decisive results. In Aikins' dictionary we are ordered to use "an extremely small quantity" of potash, and we are told that "a yellowish green precipitate" will be produced. From my own experience, I feel myself warranted to advise the addition of the larger quantity stated above, the effect of which will be a green, without any shade of yellow. In order to discover the degree of minuteness to which this test is capable of being carried, 1-40th of a grain of the white oxyde of arsenic was added to sixty grains of water, forming 1-2400 of the weight of the fluid; the proper proportion of potash and the sulphate of copper was added; and, by comparing the effect of these substances with similar quantities of potash and sulphate of copper, without the arsenic, the green was perfectly obvious. In speaking of this method of detecting arsenic, it is necessary to observe, that we must always view the fluid by reflected, and not by transmitted light, and that when we come to minute experiments, they should be examined by daylight. By the light of a candle, it is difficult to distinguish between delicate shades of blue and green, and a weak solution of the sulphate of copper, without any addition, when held between the eye and the window, frequently presents a greenish tinge. Coloured fluids are best seen by placing a sheet of white paper behind the glasses in which they are contained. To those who are not in the habit of performing chemical experiments, it may not be unnecessary to mention, that the correct method of proceeding is to begin by forming solutions of the different materials of a known strength. They are the most convenient for the purposes of calculation, when composed of one part of the solid to ten parts by weight of water, or if more water be required, to some multiple of ten, so that ten grains of the solution, or some higher decimal number, may contain exactly one grain of the substance dissolved.

The third method of detecting arsenic is the reduction of its oxyde into the metallic state, by heating it together with an inflammable body in a glass tube. The directions for performing this experiment are, that we mix the arsenic, or the suspected powder, with black flux or pounded charcoal, and put the mixture into a glass tube closed at one end, and coated with clay and sand, the other end being stopped with a plug of clay. The tube is then to be exposed to a red heat, when the arsenic will be found in the metallic state, encrusting the inside of the tube. The process seems a simple one, and it may be considered as perfectly decisive of the presence of arsenic, when the metallic coating can be observed; but there are some circumstances attending it which make it less generally applicable than might at first view be supposed. To a chemist, with a well furnished laboratory, it may seem to be starting an imaginary objection, to speak of the difficulty which there may be in procuring a glass tube of the proper size, and with a closed end; but such an occurrence may not unfrequently happen to those who are not in the habit of performing experiments; and I may add, that I apprehend few persons will, for the first time, attempt the process, who will not find themselves considerably embarrassed in the execution of it. Some of the difficulty I shall endeavour to remove, by giving a minute account of the different parts of the operation, prefacing my remarks by observing, that here, as well as in other parts of this paper, I do not profess to write for the scientific chemist. The most convenient size of the tube is about 1-4th of an inch, or rather less, in diameter, and about eight inches in length. In order to close the tube, where a blow-pipe is not to be procured (which we may suppose will often be the case) the end is to be placed in a common fire, until it is completely softened, and a pair of small tongs being at the same time made red hot, the tube is to be withdrawn from the fire, and the heated end pinched by the tongs, and at the same time bent up at an acute angle, so as to be brought parallel to the body of the tube. The tube is then to be heated a second time, and being again firmly pinched by the hot tongs, the end will be found to be completely impervious. Mr. Murray recommends that the tube be coated with clay and sand. It may be proper to mention, that this part of the process should



not be omitted, and that the best proportions for the coating are, one part of common pipe-clay to three parts of fine sand, which are to be well kneaded together, and reduced to such a state of tenacity, that the lute will readily adhere to the tube, and its different parts will unite without forming a visible seam. The black flux, which is mentioned by Mr. Murray, is often not to be procured, but powdered charcoal answers the same purpose. With respect to the quantity of the inflammable material, I think that considerably less than this author advises is more convenient, for when too much is employed, it requires a greater degree of heat, and a much longer application of it. In Aikins's dictionary we are directed to add "a little powdered charcoal." I believe that the effect will be best produced by using about equal bulks of the arsenic and charcoal. In order to get the materials into the tube, a wooden rammer must be formed, by which they may be pressed down to the closed end. A tube 1-4th of an inch in diameter, coated with a thin covering of clay and sand, requires to be kept at least 1-4th of an hour in a well burnt coal fire, in order to have the full effect produced. This method of detecting arsenic has been esteemed the most decisive, but it is by no means the most minute of the tests; when less than 3-4ths of a grain was employed, I could not say that the metallic coating was clearly perceptible. From a remark that is made by Dr. Black, it would appear, that one grain was the smallest quantity which he thought capable of detection by this process.\*

The property which arsenic possesses of uniting with copper, and forming a white compound, is the next method that has been employed to detect the presence of the former substance. To produce the effect, we are directed to place the arsenic, mixed with some carbonaceous matter, between two plates of polished copper, and having bound them together by a wire, to expose them for some time to a red heat, when the part of the copper that is in contact with the arsenic, will be found to have received a permanent white stain. The Messrs. Aikins direct the suspected powder to be mixed with a little charcoal, while Mr. Murray recommends it to be made into a paste with the black flux and oil. I should here, for the same

\* Black's Lectures, v. ii. p. 430.

reason as before, prefer the charcoal to the black flux, but I think the oil is certainly a useful addition. When the dry charcoal is employed, the powder is in danger of escaping from between the plates, unless they be very closely pressed together, and when this is the case, a considerable part of it will be found unconsumed after they have been heated. With respect to the proportion of the ingredients to be employed, I have found the process to succeed the best, when to one grain of the arsenic we add about one half grain of powdered charcoal, and about two drops of oil. This property of whitening copper is justly regarded as one of the most decisive tests of the presence of arsenic; but there are some circumstances attending it, which may deserve to be pointed out to the inexperienced. It may be necessary, in the first place, to describe the phenomena that take place when copper is heated according to the process that is described above, but without the addition of the arsenic. Two copper disks, of nearly an inch and a half in diameter, scoured bright with sand, had one grain of powdered charcoal, made into a paste with oil, placed between them; they were bound together with an iron wire, and then kept red hot for ten minutes. When they were withdrawn from the fire, the metal was found to have lost its former appearance, and to have acquired the dull white colour of lead or zinc. The insides of the disks were found to present the same whitish appearance, except on the spot where the charcoal was placed, a small part of which still remained unconsumed. As the disks cooled, the whitish matter which covered them began to separate, and fly off with some force, in the form of small scales, leaving a clean surface of the proper copper colour. In a few minutes, the greatest part of the scales were removed, except on the inside, near where the charcoal had been placed, and here the disks still retained their gray colour. The charcoal was rubbed off, and the surface below it was found smooth and polished; it had acquired a light colour, resembling that of brass, and near the centre there was a small spot, which approached to a steel gray. This appearance still continued, after it had been rubbed with fine sand. The above description of the changes which the disks experience, by being heated with a part of their surface in contact with an inflammable body, will probably impress the society with the same idea,



that, I confess, it gave to myself, that if I had performed this experiment upon a substance which was suspected to contain arsenic, and had not been aware of the appearance that I was to meet with, I should have conceived that I had detected its presence. Upon repeating the process, in precisely a similar manner, except that one grain of arsenic was added to the charcoal, the oxydation of the copper took place as before, and a small part of the charcoal remained unconsumed; but upon rubbing it the white stain was perfectly visible. However, when these disks were compared with those in which the former experiment had been made, the difference between them seemed more in *degree* than in *kind*, so that I should not choose to decide upon the presence of arsenic as indicated by this test, unless the result was more obvious than it must be expected to be, when it exists only in small quantity. It may be proper to observe, that copper whitened in this manner by arsenic, is very subject to tarnish; in three days I could with difficulty distinguish which of the disks had been employed in these two experiments.

With respect to the 5th method that has been pointed out for detecting arsenic, by exposing it to a high temperature, and observing the white smoke and peculiar odour which arise from it in this situation, I shall not think it necessary to make many remarks. It is certainly not much to be depended upon; unless the arsenic is in considerable quantity, the effects are not sufficiently perceptible; and if it be mixed with either an animal or a vegetable substance, the smoke and smell arising from these bodies, when heated, will altogether prevent us from detecting the peculiar properties of the arsenic. When a quantity of arsenic is mixed with an equal weight of flour, and placed upon iron at a low red heat, so as not to cause the flour to inflame, the suffocating smoke that arises from the flour can alone be perceived; nor is it possible to discover that any thing has been mixed with it. With respect to the degree of heat, at which the characteristic odour of arsenic is most readily discovered, by having a bar of iron heated red hot at one end, and becoming gradually cooler towards the other extremity, I thought that the effect was the most perceptible in that part which was just visibly red in bright daylight.

After this review of the different processes for detecting

arsenic, it remains for us to determine on which of them the greatest confidence may be placed. I have no hesitation in giving it as my opinion, that the most convenient, the most delicate, and the most decisive process, is the one in which the green precipitate is formed by the addition of the sulphate of copper. The arsenic may be detected with certainty in a very minute quantity, as well in a fluid as in a solid state; no apparatus is requisite, nor is there any skill required on the part of the operator. When we are called upon, in a judicial investigation, to ascertain whether arsenic has been employed, we are generally obliged to perform our experiments on a small quantity of the substance which may be accidentally left in the cup or bottle, or to search for it among the contents of the stomach. In this latter case, particularly, it is only in minute quantity that we can expect to find it; and in order to separate it from the mass in which it is contained, we must have recourse to the solvent power of hot water.

There are two precautions which I should strongly recommend to every person who may be called upon to search for arsenic in a suspected substance. The first is, that in every experiment which is performed upon the unknown substance, a corresponding one should be made with ingredients of a known composition. If, for example, the test of the sulphate of copper be applied to a suspected fluid, the operator should at the same time add the potash and sulphate of copper to a portion of pure water, and afterwards he should repeat the process with the potash, sulphate of copper, and a given quantity of an arsenical solution. The effect of the reagents will thus become familiar to him both with and without the arsenic, and he will then be enabled, with much more confidence, to state his opinion respecting the fluid under consideration. The second recommendation which I shall beg leave to offer, is, that the examination be never entered upon, until the substances that are intended to be employed as tests, and the necessary apparatus, are all in readiness, and all in a perfect state. In experiments of this kind, there should be *no allowances* made; and as it often happens that the quantity of the matter to be operated upon is small, none of it ought to be wasted upon imperfect trials.

Liverpool, February 10, 1809.



*The Croonian Lecture.*

BY WILLIAM HYDE WOLLASTON, M. D.

From Philosophical Transactions for 1810. Part I.

I AM aware that the remarks which I have to offer on the present occasion, may be thought to bear too little direct relation to each other for insertion in the same lecture; yet any observation respecting the mode of action of voluntary muscles, and every inquiry into the causes which derange, and into the means of assisting the action of the heart and blood-vessels, must be allowed to promote the design of Dr. Croone, who instituted these annual disquisitions. And it has always appeared to be one great advantage attending the labours of this society, that it favours the production of any original knowledge, however small, in a detached form; and enables a writer to say all that he knows upon a particular subject, without inducing him to aim at the importance of a long dissertation. I shall therefore make no apology for dividing the following lecture into three distinct parts.

In the first of which I shall treat of the duration of voluntary action.

In the second I shall attempt to investigate the origin of seasickness, as arising from a simple mechanical cause deranging the circulation of the blood.

In the third, I shall endeavour to explain the advantage derived from riding, and other modes of gestation, in assisting the health under various circumstances, in preference to every species of actual exertion.

PART I. ON THE DURATION OF MUSCULAR ACTION.

The necessity of occasional intermissions from a series of laborious exertions, is within the experience of every one; the fatigue of continuing the effort of any one voluntary muscle without intermission even for a few minutes is also sufficiently known; but there is a third view of the duration of muscular action which appears to have escaped the notice of physiologists. For I believe it has not hitherto been observed that each effort, apparently single, consists in reality of a great number of contractions repeated at extremely short intervals; so short



indeed that the intermediate relaxation cannot be visible, unless prolonged beyond the usual limits by a state of partial or general debility.

I have been led to infer the existence of these alternate motions from a sensation perceptible upon inserting the extremity of the finger into the ear. A sound is then perceived which resembles most nearly that of carriages at a great distance passing rapidly over a pavement.

The rapidity of the motion varies according to the degree of force with which the finger is retained in its place. The sound thus perceived is not at all dependent on the degree of pressure upon the tympanum; for, on the contrary, the vibratory sound is most distinct when that pressure is slight, if the finger be at the same time rendered rigid by the forcible action of antagonist muscles; and when the ear is stopped with great force without the presence of muscular action, no such sound is produced. For instance, if the head be rested upon the hand, in such a position as to press with its whole weight upon the ball of the thumb applied to the ear, no noise is perceived unless the extremity of the thumb be at the same time pressed against the head, or unless the action of some other muscles be communicated to the ear, by any inadvertence in the method of conducting the experiment.

When I endeavoured to estimate the frequency of these vibratory alternations, they appeared to be in general between twenty and thirty in a second; but it is possible that the method I employed may be found defective, and it is to be hoped that my estimate may be corrected, by some means better adapted to the determination of intervals that cannot actually be measured.

It was by imitation alone that I was enabled to judge of their frequency. For this purpose I contrived to render the vibration itself, and the imitative sound, both audible by the same ear.

While my ear rested on the ball of my thumb, my elbow was supported by a board lying horizontally, in which were cut a number of notches of equal size, and about one-eighth of an inch asunder. Then, by rubbing a pencil or other round piece of wood with a regular motion along the notches, I could imitate pretty correctly the tremor produced by the pressure of my thumb against my head; and by marks to indicate the

number of notches passed over in five or ten seconds, observed by my watch, I found repeated observations agree with each other as nearly as could be expected; for I could not depend upon exerting the same degree of force in different trials.

That I might not be deceived by the resemblance of tremors, which coincided only at alternate beats, and therefore might be considered as octaves in music to each other, I sometimes employed notches at greater and sometimes at less distances from each other, but the result was nevertheless the same; and in order to avoid any error that might be caused by some accidental quality of the sound arising from the length of the muscle employed, or length of the bones concerned in conveying the imitative sound to my ear, I made the following variation of the experiment. My ear was stopped by a cushion pressed upon by the end of a notched stick that rested on my foot, and thus conveyed the vibration from the muscles of my leg to the ear, along with the tremor produced by friction upon the notches; and still the results were nearly the same; varying in frequency between twenty and thirty in a second, according to the degree of force exerted in the experiment.\*

As a further proof that I was not much deceived in my judgment of the frequency of these vibrations, I requested two or three of my friends to repeat the same experiment for me, and our agreement was such as to confirm me in opinion, that there could be no very considerable error in the estimate.

The greatest frequency that I think I have observed, was about thirty-five or thirty-six in a second, and the least was as low as fourteen or fifteen; but in attempting to lessen the number of vibrations, there appears to be a degree of unsteadiness which prevents any accurate measurement of the real number.

It is very probable, that in cases of great debility the number may be even considerably less, and may be the reason of that

\* The resemblance of the muscular vibrations to the sound of carriages at a distance, I apprehend to arise not so much from the quality of the sound, as from an agreement in frequency with an average of the tremors usually produced by the number of stones in the regular pavement of London, passed over by carriages moving quickly.

If the number of vibrations be supposed twenty-four in a second, and the breadth of each stone be about six inches, the rate of a carriage thus estimated would be about eight miles an hour; which agrees with the truth as nearly as the assumptions on which the estimate is founded.



visible unsteadiness, which is known to occur in persons enfeebled by age, or much reduced by disease.

Possibly the foregoing observation may not be altogether new to some members of this society, as it is now about seventeen or eighteen years since it first occurred to me, and I was then accustomed occasionally to mention it in conversation with my friends; but I am not aware that any other person has made the same remark respecting the vibratory nature of muscular action, although I find that Grimaldi had observed the sound that occurs upon stopping the ears, but ascribed, it according to the notions that prevailed in his time, to the hurried motions of the animal spirits.\*

#### PART II. ON SEA-SICKNESS.

The second remark which I have to offer to the society relates to sea-sickness, the cause of which has not hitherto been fully explained; and although the explanation which I am about to propose, may not appear altogether satisfactory to persons who, when at sea, are also rendered giddy by the incessant motion of the waves, and are consequently liable to consider as cause and effect phenomena which in their minds are constantly associated; yet the observation on which it is founded may deserve to be recorded, on account of the degree of relief that may be obtained in that most distressing affection.

After I had been harassed by sea-sickness during a short voyage for some days, and had in vain attempted to account for the difference between the inexperienced passenger, and those around him more accustomed to the motion of the sea, I imperceptibly acquired some power of resisting its effects, and had the good fortune to observe a peculiarity in my mode of respiration, evidently connected with the motion of the vessel, but of which, in my then enfeebled state, I was unable to investigate either the cause or consequence. In waking from a state of very disturbed sleep, I found that my respirations were not taken with the accustomed uniformity, but were interrupted by

\* Vera itaque ratio experimenti prædicti est, quia in digito et brachio totoque corpore continuo fiunt multi motus ac tremores, ob spirituum agitationem huc illuc perpetuo accurrentium. *Grimaldi*, *Physicomathesis de Lumine*, p. 383.



irregular pauses, with an appearance of watching for some favourable opportunity for making the succeeding effort; and it seemed as if the act of inspiration were in some manner to be guided by the tendency of the vessel to pitch with an uneasy motion.

The mode by which I afterwards conceived that this action could primarily affect the system, was by its influence on the motion of the blood; for, at the same instant that the chest is dilated for the reception of air, its vessels become also more open to the reception of the blood, so that the return of blood from the head is more free than at any other period of a complete respiration. On the contrary, by the act of expelling air from the lungs, the ingress of blood is so far obstructed, that, when the surface of the brain is exposed by the trepan, a successive turgescence and subsidence of the brain is seen, in alternate motion with the different states of the chest. It is probably from this cause that, in severe head-aches, a degree of temporary relief is obtained by occasional complete inspirations.

In sea-sickness also the act of inspiration will have some tendency to relieve, if regulated so as to counteract any temporary pressure of blood upon the brain; but the cause of such pressure requires first to be investigated.

All those who have ever suffered from sea-sickness (without being giddy) will agree that the principal uneasiness is felt during the subsidence of the vessel by the sinking of the wave on which it rests. It is during this subsidence that the blood has a tendency to press with unusual force upon the brain.

If a person be supposed standing erect upon deck, it is evident that the brain, which is uppermost, then sustains no pressure from the mere weight of the blood, and that the vessels of the feet and lower parts of the body must contract, with a force sufficient to resist the pressure of a column of blood, of between five and six feet from the head downwards.

If the deck were by any means suddenly and entirely removed, the blood would be no longer supported by its vessels; but both would fall together with the same velocity by the free action of gravity; and the same contraction of the vessels which before supported the weight of the blood would now occasion it to press upon the brain, with a force proportional to its former altitude.

In the same manner, and for the same reason, during a more gradual subsidence of the deck, and partial removal of support, there must be a partial diminution of the pressure of the blood upon its vessels, and consequently a partial reaction upon the brain, which would be directly counteracted by a full inspiration.

The consequence of external motion upon the blood will be best elucidated by what may be seen to occur in a column of mercury similarly circumstanced.

A barometer, when carried out to sea in a calm, rests at the same height at which it would stand on shore; but, when the ship falls by subsidence of the wave, the mercury is seen apparently to rise in the tube that contains it, because a portion of its gravity is then employed in occasioning its descent along with the vessel; and accordingly, if it were confined in a tube closed at the bottom, it would no longer press with its whole weight upon the lower end. In the same manner, and for the same reason, the blood no longer presses downwards with its whole weight, and will be driven upwards, by the elasticity which before was merely sufficient to support it.

The sickness occasioned by swinging is evidently from the same causes as sea-sickness, and that direction of the motion which occasions the most piercing sensation of uneasiness, is conformable to the explanation above given.

It is in descending forwards that this sensation is perceived; for, then the blood has the greatest tendency to move from the feet towards the head, since the line joining them is in the direction of the motion. But when, in the descent backwards, the motion is transverse to the line of the body, it occasions little comparative inconvenience, because the tendency to propel the blood towards the head is then inconsiderable.

The regularity of the motion in swinging, afforded me an apparently favourable opportunity for trying the effect of inspiration; but although the advantage was manifest, I must confess, it did not fully equal the expectations I had formed from my experience at sea. It is possible that the suddenness of the descent may in this case be too great to be fully counteracted by such means; but I am inclined to think that the contents of the intestines are also affected by the same cause as the blood; and if these have any direct disposition to regur-



gitate, this consequence will be in no degree counteracted by the process of respiration.

A friend of mine informed me that he had endeavoured to counteract this mechanical effect upon the stomach, and had experienced immediate relief from a slight degree of sea-sickness, by lying down upon the deck with his head towards the stem of the vessel; by means of which, upon pitching, he was in the attitude of a person descending backwards in a swing.

Whether the stomach be or be not thus primarily affected, or only by sympathy with the brain, the sensation of sinking is in all cases referred directly to the stomach, which is seized with such instantaneous retching, that no person who has not been so situated can form a just conception of it.\*

In thus referring the sensations of sea-sickness in so great a degree to the agency of mere mechanical pressure, I feel confirmed by considering the consequence of an opposite motion, which, by too quickly withdrawing blood from the head, occasions a tendency to faint, or that approach to fainting, which amounts to a momentary giddiness, with diminution of muscular power. At a time when I was much fatigued by exercise, I had occasion to run to some distance, and seat myself under a low wall for shelter from a very heavy shower. In rising suddenly from this position I was attacked with such a degree of giddiness, that I involuntarily dropped into my former posture, and was instantaneously relieved, by return of blood to the head, from every sensation of uneasiness.

\* There is one occasion upon which a slighter sensation of this kind is perceived, and it appears to indicate the direction of the motion from which it arises, to be downwards. "In a country subject to frequent returns of earthquakes," it is said (*Phil. Trans.* vol. xliii. p. 41.) that "a few minutes before any shock came, many people could foretel it by an alteration in their stomachs; an effect which (it is added) always accompanies the wave-like motion of earthquakes, when it is so weak as to be uncertainly distinguishable." (*Michell, Phil. Trans.* vol. li. 610.)

It seems that the vapours to which these tremendous concussions are owing, immense in quantity, and of prodigious force, being for a time confined on all sides, elevate the surface of a country to a vast extent, until they either find vent, or meet with some partial cause of condensation; and hence the alternate heaving and subsidence of the ground will produce much the same effects as the rising and falling of the swell at sea.



Since that time, the same affection has frequently occurred to me in slighter degrees, and I have observed, that it has always been under similar circumstances of rising suddenly from an inclined position, after some degree of previous fatigue. Sinking down again immediately removes the giddiness; and then, by rising a second time more gradually, the same sensation is avoided.

PART III. ON THE SALUTARY EFFECTS OF RIDING, AND OTHER  
MODES OF GESTATION.

In the preceding instances of disturbing the circulation of the blood, by external motion, the effect is disagreeable, and proportionally prejudicial. There may indeed be cases of disorder, in which it will be salutary, but these are probably less frequent than is generally supposed.

In the observations which follow, general opinion will concur with me, on the benefit derived from external or passive motion, and I hope that, in ascribing its good effects to their true cause, I shall enable others to make a valuable distinction, which has not yet been preserved with due care, between one motion which is salutary, and another which is very frequently pernicious. For, although the term *gestation* is employed by medical writers, as a general term comprehending riding on horseback, or in a carriage, and although the merits of such motions, especially the former, were clearly noticed, and perhaps even over-rated, by the discernment of Sydenham, I believe that no explanation has yet been given, of the peculiar advantages of external motion, and am persuaded, that the benefits to be derived from carriage exercise are by no means in so high estimation as they ought to be.

Under the common term *exercise*, active exertion has too frequently been confounded with passive gestation, and fatiguing efforts have consequently been substituted for motions that are agreeable, and even directly invigorating, when duly adapted to the strength of the invalid, and the peculiar nature of his indisposition.

The explanation which I am about to offer of the effects of external motion upon the circulation of the blood, is founded upon a part of the structure observable in the venous system, the mechanical tendency of which cannot be doubted. The

valves which are every where dispersed through those vessels, allow free passage to the blood, when propelled forward by any motion that assists its progress; but they oppose an immediate obstacle to such as have a contrary tendency. The circulation is consequently helped forward by every degree of gentle agitation. The heart is supported, in any laborious effort that may have become necessary, by some obstacle to its exertions; it is assisted in the great work of restoring a system, which has recently struggled with some violent attack: or it is allowed, as it were, to rest from a labour, to which it is unequal, when the powers of life are nearly exhausted by any lingering disorder.

In the relief thus afforded to an organ so essential to life, all other vital functions must necessarily participate; and the various offices of secretion and assimilation, by whatever means they are performed, will not fail to be promoted during such comparative repose from laborious exertion.

Even the powers of the mind itself, though apparently least likely to be influenced by mere mechanical means, are manifestly, and in many persons most immediately, affected by these kinds of motion.

It is not only in cases of absolute deficiency of power to carry on the customary circulation, that the beneficial effects of gestation are felt, but equally so when comparative inability arises from redundancy of matter to be propelled. When from fulness of blood the circulation is obstructed, the whole system labours under a feeling of hurry and agitation, with that sensibility to sudden impressions which is usually termed nervousness. The mind becomes incapable of any deliberate consideration, and is impressed with horrors that have no foundation but in a distempered imagination.

It is in moderate degrees of this species of affection that the advantages of carriage exercise are most sensibly felt. The composed serenity of mind that succeeds to the previous alarm, is described by some persons with a degree of satisfaction that evinces the decided influence of the remedy. With this steadier tone of mind, returns its full power of cool reflection; and if the imagination becomes more alive than usual, its activity is now employed in conceiving scenes that are amusing and agreeable.



As an instance of direct relief to a circulation labouring from mere fulness of blood, I may adduce that of a person, whose friends, as well as himself, were apprehensive, from the violent and visible throbbing of his heart, of the existence of some organic mischief, and were in some measure alarmed for the consequences.

He was persuaded and not reluctantly, to go without delay for medical advice, and was accordingly conveyed in a carriage to the house of some physician of eminence, but did not succeed in finding him at home. As the symptoms did not appear to admit of delay, and were at least not aggravated by the motion, it was hoped that the wished-for advice might be obtained at a part of the town which happened to be at some distance. But the second attempt proved as fruitless as the former, and a third was made with the same event. Since the throbbing had by that time considerably abated, he was contented to postpone any further efforts to the following day, and directed the carriage homewards. By the time that he returned to his friends, he found that the motion of travelling over several miles of pavement had apparently removed the complaint. The pulsation of the heart and arteries had subsided to their natural standard, and he congratulated himself, that his search of a remedy had not been ineffectual, although he had been disappointed as to the source from which he thought he had most reason to expect relief.

If vigour can in any instance be directly given, a man may certainly be said to receive it in the most direct mode, when the important service of impelling forward the circulation of his blood is performed for him by external means. The main spring, or first mover of the system, is thereby, as it were, wound up; and although the several subordinate operations of so complicated a machine cannot be regulated in detail by mere external agency, they must each be performed with greater freedom, in consequence of this general supply of power.

In almost every treatise on the subject of chronical diseases, are to be found numerous instances of the benefit produced by the several modes of gestation which have been most generally adopted; as riding on horseback, in carriages, sea-voyages, and swinging. And in many cases which might be adduced, it has appeared too clear to admit of a doubt, that the cure of the pa-



tient has been owing *solely* to the external agitation of his body, which must be allowed, at least, to have had the effect above explained; that of relieving the heart and arteries from a great part of their exertion in propelling the blood, and *may* therefore have contributed to the cure, by that means only.

The different modes above mentioned are adapted from their nature to different degrees of bodily strength; and if there are cases in which that which appears most eligible may not suit the situation or circumstances of the patient, it cannot be difficult to contrive other means of giving motion, so as least to incommode, and yet to give the greatest relief. A very gentle and long continued, or even incessant motion, may suit some cases better than any more violent and occasional agitation; and in this way, probably, it is, that sea-voyages have sometimes been attended with remarkable advantage.

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*Hints on the subject of Animal Secretions.*

BY EVERARD HOME, ESQ. F. R. S.

Communicated by the Society for the Improvement of Animal Chemistry.\*

From Philosophical Transactions for 1809, part II.

THE brilliant discoveries of Mr. Davy on the powers of electricity in producing chemical changes, suggested to me the idea that the animal secretions may be produced by the same means.

\* Dr. Wollaston's observations inserted in the Philosophical Magazine, were published after this paper had been laid before the society.

I was led to the present investigation, while preparing my lectures on the Hunterian Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. William Brande to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir Joseph Banks, and stated that I should bring them forward in my lectures; at that time Dr. Young's Syllabus was not published, and Dr. Wollaston's opinions were unknown to me.

Dr. Berzelius, professor of chemistry at Stockholm, published a work on Animal Chemistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of his opinion, the following experiment is adduced:

"Trace all the nerves leading to any secretory organ in a living animal;

To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chemistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this society, the object of which is the improvement of animal chemistry. Their intimate acquaintance with these branches of science renders them peculiarly fitted for such an undertaking.

It is one of the most important subjects to which Mr. Davy's discoveries can be applied, and he has given it the consideration it deserves.

The Voltaic battery is met with in the torpedo and electrical eel; and although it is given only as a means of catching their prey, and defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts: one, that a Voltaic battery can be formed in a living animal; the other, that nerves are essentially necessary for its management; for, in these fish, the nerves connected with the electrical organs exceed those that go to all the other parts of the fish in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a Voltaic battery of high power; but their structure appears to Mr. Davy to adapt them to receive and preserve a small electrical power.

That the nerves arranged with muscles, so as to form a Voltaic battery, have a power of accumulating and communicating electricity, is proved by the well-known experiment of taking the two hind legs of a vivacious frog, immediately after they are cut off, laying bare the crural nerves, applying one of these to the exposed muscles of the other limb, and then when

and divide them, being careful to injure the blood vessels and the structure of the organ itself, as little as may be: notwithstanding the continued circulation of the blood, the organ will as little secrete its usual fluid, as an eye deprived of its nerve can see, or a muscle whose nerve has been divided can move. We may therefore easily conceive, that any trifling alteration in the nerves of a gland may materially affect its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting, when we discover the insufficiency of all other explanation. Dr. Berzelius's work was shown to me by Mr. Davy while this paper was in the press.



the circle is completed by raising the other crural nerve with a glass rod, and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs. The ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles, and the course of the nerves of the viscera which keep up a connexion among themselves in so many different ways.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. W. Brande, who has begun his career in animal chemistry with so much success, made the following experiments, in the suggestion of which Mr. Davy afforded him every assistance.

#### EXPERIMENT I. Middle of January, 1809.

The conductors from twenty-four four-inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been withdrawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire was of a deep red colour and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.



In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

EXPERIMENT II. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the meso-colon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three-inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of an hour; a third ligature was then tied in the centre of the detached vein, in order to cut off the connexion between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long-continued electrical action, the serum only was employed in the following experiments.

EXPERIMENT III. 10th of March, 1809.

The conductors from one hundred and twenty four-inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

EXPERIMENT IV. 14th of April, 1809.

Was undertaken to ascertain the effect of a low power: a

battery was employed, consisting of twelve four-inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole; in five minutes, the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in length was seen floating in the fluid, and adhering to the same wire.

#### EXPERIMENT V. 6th of May, 1809.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three-inch plates very *weakly* charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing, it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid, (super salts.)

By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form; with a less degree, it is separated in a fluid form.

From these facts, the following queries are proposed:

1st. That such decomposition of the blood by electricity, may be as near an approach to secretion as could be expected to be produced by the artificial means at present in our power.

2d. That a weaker power of electricity, than any that can be readily kept up by art, may be capable of separating from the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose; and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.



4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed.

5th. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6th. As albumen becomes visibly coagulated, by the effect produced from twelve four-inch double plates of copper and iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chemical test of electricity, whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.



## SELECTED REVIEWS.

*Report of the Royal College of Physicians of London on Vaccination; with an Appendix, containing the Opinions of the Royal Colleges of Physicians of Edinburgh and Dublin, and of the Royal Colleges of Surgeons of London, of Dublin, and of Edinburgh. Ordered to be printed, 8th July 1807. pp. 13. Folio.*

*A Letter, in reply to the Report of the Surgeons of the Vaccine Institution, Edinburgh; with an Appendix, containing a variety of interesting Letters on the Subject of Vaccination, and including a Correspondence with Dr. Duncan, Dr. Lee, and Mr. Bryce: from which also the public will be able to appreciate the Authority of the Surgeons of the Vaccine Institution, and to form a correct opinion of the whole subject. By Thomas Brown, Surgeon, Musselburgh, 8vo. Edinburgh, 1809. And other publications on Vaccination.*

From the Edinburgh Review for January 1810.

ON a former occasion we entered at considerable length into the merits of Vaccination, and gave a sketch of the acrimonious controversy it had excited in the medical world. Since that time the subject has acquired still greater interest, and has given rise to various legislative projects of no common delicacy and importance. It is very probable, we think, that these will soon be renewed and multiplied; and, in disposing of them we are well aware, that the utmost caution will be requisite; lest on the one hand we prevent the adoption of safe and effectual means, for limiting the ravages of the most loathsome and destructive of our diseases—or, on the other, increase, by precipitate coercion, the evils we wish to avert.

The long list of publications prefixed to this article, and which, after all, is but a small portion of what has lately been written on the subject, will give our readers an idea of the keenness with which the vaccine controversy is still maintained. While the vaccinists have been strengthening their position by parliamentary votes and collegiate reports;—while Mr. Blair has been flogging Mr. Birch, and Mr. Ring has been sounding a peal in the ears of Dr. Mosely,—the nicknames, handbills, squibs, and caricatures of their adversaries, have been too suc-

cessfully employed, where they were calculated to do most mischief—among the weak and the ignorant. This disgraceful warfare, we must however remark, has been almost entirely confined to the metropolis; and, till within these few months, was unknown on this side of the Tweed.

It would be both an irksome and unprofitable task, to attempt to give a regular view of all that has been written for and against cow-pox, since the subject was formerly under our consideration. With a few praiseworthy exceptions, the dispute has been carried on with the same violence and disregard of accuracy, either in observing or in reasoning, which we had formerly so much occasion to reprobate. In truth, vaccination has had to struggle, not only against the indefatigable activity of avowed opponents, but also against the treacherous manœuvres of pretended friends, and the misguided zeal of injudicious partisans. While Dr. Auban recommends vaccination to the followers of Mahomet as a preventive, not only of small-pox, but also of the plague, Dr. Gillum, in the course of his arguments for a *gradual* introduction of cow-pox, seriously expresses his fears to lord Hawkesbury, that by relinquishing small-pox inoculation, we shall have the plague again introduced among ourselves. This sagacious conjecture is assumed as a fact in another lucubration of the same author,—“On the Efficacy of inoculated Small-Pox in *promoting the Population of Great-Britain.*”—“Inoculation,” observes this profound and patriotic writer, “has supplied mild small-pox, and consequently saved to the country the multitude of inhabitants formerly destroyed by the plague. Hence, had inoculation been known, and seasonably employed, the plague of London in 1665 might have been prevented, and the lives of 100,000 of its inhabitants saved, exclusive of their offspring.”

“Et nati natorum, et qui nascuntur ab illis.”

But, leaving Dr. Gillum to his profound meditations, it is time for us to enter upon the cardinal point of the controversy, which is, whether vaccination be an effectual preventive of small-pox or not. This, it is evident, is entirely a question of fact; and will be more satisfactorily answered, in proportion to the extent and uniformity of our actual experience and observation. In both respects, we do not know almost any disputable point, certainly not one in the whole range of medical sci-



ence, of which the affirmative is as decisively proved. The great majority, we may say all the respectable part of the profession, now concur in considering vaccination as a safe and perfect security against small-pox; in recommending and promoting it in evident opposition to their private interest; and in intrusting to it, in full confidence of its efficacy, not merely the lives of their patients, but of their children and dearest relatives. Those who received the first accounts of it with most scepticism and doubt, are now its firmest patrons and advocates; and, in this instance, the young and the enthusiastic have been ultimately supported by the testimony of the old and experienced. In proof of this, we need only quote the concluding paragraph of the report of the London College of Physicians, who, under the authority of parliament, endeavoured to collect the opinion of the profession at large.

"From the whole of the above considerations, the College of Physicians feel it their duty strongly to recommend the practice of vaccination. They have been led to this conclusion by no preconceived opinion, but by the most unbiassed judgment, formed from an irresistible weight of evidence which has been laid before them. For when the number, the respectability, the disinterestedness, and the extensive experience of its advocates are compared with the feeble and imperfect testimonies of its few opposers; and when it is considered that many, who were once adverse to vaccination, have been convinced by further trials, and are now to be ranked among its warmest supporters, the truth seems to be established as firmly as the nature of such a question admits; so that the College of Physicians conceive, that the public may reasonably look forward, with some degree of hope, to the time when all opposition shall cease, and the general concurrence of mankind shall at length be able to put an end to the ravages, at least, if not to the existence, of the small-pox."

The detailed and valuable report of the Central Society to the minister of the interior of France, is not less satisfactory; nor has any feeling of national rivalry prevented them from owning their obligations to this country for one of the most unexpected and beneficial discoveries ever made in the art of medicine. We could easily quote similar testimonies, in favour of vaccination, from every medical board in Europe. The remo-



test corners of the globe, indeed, have now experienced its efficacy. In every country into which it has been introduced, its progress has been uniform and steady; and no experiment, perhaps, was ever conducted on so extensive a scale, nor any discovery so rapidly disseminated. We have no *data* to estimate correctly the number of individuals of every race, and of every climate, who have been vaccinated; or to ascertain the proportion of favourable cases: but these are not necessary in order to enable us to form a decisive opinion upon the great questions regarding vaccination. A remedy, a mode of practice, or an opinion, may become fashionable in *one* country from adventitious circumstances; but they will not make their way in *all* countries, and under all variety of circumstances. Vaccination, however, has penetrated to the remotest corners of the globe; and, wherever it has been introduced, the increasing experience of every year has only served to confirm the general confidence in its efficacy. It is impossible to explain these facts upon any other principle, than that the advantages derived from it are substantial and permanent. Simple as the practice is, it is attended with some trouble; and nothing but a very firm and general conviction of its utility, could induce the bulk of the population of any country, much less of so many countries, to submit to it.

Nor is the great extent of the practice to be explained, by ascribing it to any undue influence of professional men. Their persuasions might have considerable weight within a certain circle of friends and patients; and imitation might lead a considerable number more to follow their example: but we repeat, that nothing but a conviction of its utility could have induced so large a proportion of the inhabitants of the world to receive and adopt it. Even the patronage it has experienced from medical men of every description, is conclusive in its favour. When first promulgated, it was received with scepticism and distrust; its phenomena were strictly investigated; and its reputed anti-variolaous powers repeatedly exposed to the severest tests, until all doubts were removed. In Scotland, we know of only *one* medical man who is not satisfied with it, and who does not recommend or practise it; and the same gentleman is the only individual of the profession, in any country, who, as far as we have learned, has abandoned it, after having been satisfied, or

rather, as we shall presently see, pretending to have been satisfied of its efficacy. It must also be remembered, that the general practice of vaccination is injurious to the pecuniary interests of the profession; and therefore, the patronage bestowed upon it by them is a most honourable proof of the candour and disinterestedness of the profession at large. We have heard a great deal of railing about jobs and jobbing; and Mr. Brown has insinuated, "that the motives of its greatest advocates are not more disinterested than those of its greatest enemies." But although it might be granted that a few individuals may have been actuated by the desire of notoriety,—by a sure introduction into practice,—or by the hopes of being appointed to a lucrative office in the vaccine institutions, still, the motives of the great majority of the profession, are evidently above all suspicion. The inoculator cannot expect the same remuneration for performing an apparently easy operation, for the event of which the most timid mother has not a moment's anxiety, as for conducting his patient safely through a painful, loathsome, and dangerous disease. Vaccination has even cut off entirely a very considerable source of the professional emoluments of the physician. Natural small-pox is entirely banished from the higher and middling classes of society; and the cow-pox is too insignificant a complaint to require the consultation of an extraordinary medical attendant. But, although vaccination be adopted and recommended by far the greatest and most respectable part of the profession, still it has been most obstinately opposed by a few individuals, and by means the best calculated to make an impression on the timid and ignorant. We shall now examine the grounds of their opposition, and their pretensions to our notice. They contend that vaccination does not afford sufficient security against small-pox, that it has injurious effects on the constitution; and that it has put an end to inoculation, without serving the same purposes.

The first is the most important objection; not on account of its being better founded than the others, but on account of the nature of the evidence brought in support of it; which consists in a multiplicity of statements, originating in misconception or misrepresentation, and not admitting of a general and permanent refutation. One fact is no sooner explained or contradicted, than another is invented.



The boldest and most determined antivaccinists of the present day, however, will scarcely venture to maintain, as they did once, that cow-pox affords no security whatever. The thousands, and tens of thousands, in every quarter of the world, whom it has enabled to resist variolous contagion,—the total extinction of that pestilence in whole countries, and the security against its importation, which they have experienced from there being no subjects for it to operate upon,—leave no doubt upon the subject. Hence, most of those who originally maintained that opinion, in the most positive and peremptory manner, have now shifted their ground; and contend, that though vaccination must be allowed to impart security *for a time*, this security gradually decays, and is at last exhausted. This hypothetical objection was satisfactorily refuted long ago by Drs. Willan and Stanger. The latter found, by direct experiment, that the insertion of variolous matter into the arms of twenty children, who had been vaccinated in 1801, produced exactly the same effects in 1802 and in 1804. This same opinion, however, has been lately brought forward, with much parade and pretension, by Mr. Thomas Brown, surgeon in Musselburgh, as if it were something new and incontrovertible. The sensation produced by the vaunting advertisements of this gentleman's book in the public papers, induced the managers of the Public Dispensary and Vaccine Institution, to make a strict inquiry into the cases he had referred to; and a report from the surgeons of that useful charity was drawn up and published. Mr. Brown has since replied, both in a pamphlet and in the newspapers; and we shall now proceed to examine his statements and arguments a little more closely, both because they are the most recent and the most detailed that have been laid before the public, and because the reputed facts having taken place in our immediate neighbourhood, are more easily inquired into.

Before we can admit that any instance of unsuccessful vaccination has been established, we must be satisfied of two things: *first*, that the patients were properly vaccinated; and, *secondly*, that they were afterwards affected with the small-pox.

The difficulty of ascertaining the former of these circumstances, is the chief cause of what are called cow-pox failures. "There is little doubt," say the London College, "that some



of the failures are to be imputed to the inexperience of the early vaccinators." And, indeed, when we consider that, from the very nature of the cow-pox, the distinction between a mere local affection, affording no security even for a day, and a perfect constitutional affection, is so small, as to require the utmost attention on the part of the most experienced; that, at the beginning of the practice, all the necessary minutiae were not thoroughly understood; that from the simplicity and safety of the operation, ladies and clergymen, midwives and farriers, vied with each other in multiplying their inoculations: And when we also consider that the total number of those vaccinated in Great Britain, during the few years which have elapsed since the promulgation of the discovery, is perhaps not less than those variolated since inoculation was first introduced, it is so far from being surprising that some failures have taken place, that we ought rather to wonder that they have been so few. Even now, some practitioners seem to be ignorant of what is necessary to constitute constitutional cow-pox. Of this number we cannot help suspecting, is our Scottish antivaccinist, who, at the same time, displays considerable ignorance of the phenomena of small-pox. "In conformity, then," says he, "both with my own experience of the phenomena of inoculation and vaccination, I contend, that if you have a vesicle, attended with an areola, you may depend upon the production of whatever effects it is capable of." Some of the other antivaccinists go still farther, and represent every puncture by a lancet armed with vaccine virus, whatever effect it produce, or even if it should not produce any effect at all, as a true vaccination, for which the supporters of the practice must be responsible, in case small-pox should supervene.

The testimony of persons entertaining such opinions cannot possibly be admitted, as to the fact of the vaccination having been complete, even when they have had an opportunity of witnessing the progress of the vesicle. The cases, which they state on hearsay evidence, and on the authority of ignorant mothers, of course are still more suspicious; and in truth, there is no tale so improbable, no story so palpably absurd, but, provided it be adverse to vaccination, it is immediately received with open ears and willing hearts, by Drs. Mosely and Squirrel, and Messrs. Birch and Lipscomb. If it militate

against cow-pox, it must be true; and it is needless to investigate it. Mr. Brown, we must do him the justice to say, is honest or hardy enough to confess this.

"It is perhaps proper to notice, that I carefully avoided having any communication with the different practitioners by whom the children were vaccinated, being aware of the strong hold which system has upon the human mind; more especially in this very important subject; thinking it best for the attainment of truth, to state with the greatest possible accuracy, the parents' account of the circumstances that attended the vaccination of their children, notice the appearance of the arm, and give the names of the families, whereby an opportunity is afforded to any of the practitioners to satisfy themselves."

In his reply to the surgeons of the vaccine institution, he betrays still less reserve in maintaining the same doctrine. Some of his statements with regard to certain cases of supposed failure at Haddington, having been contradicted by the medical gentleman who had the care of them, with a civil expression of his regret that Mr. Brown had not applied to him *before* publishing his book, that learned person most valiantly replies—

"Far from regretting that I did not communicate with the medical gentlemen at Haddington, before I mentioned in my book that such cases had occurred there, I have daily reason to congratulate myself on the opinion I had formed of the extensive and alarming effects of system on the human mind; for, had I done so, and afterwards had been regulated by the information I certainly would have received, undoubtedly no such opinions would have been promulgated; and which," says Mr. Brown, "I now find many very respectable characters, both in and out of the profession, consider as entitled to attention and respect."

Such a confession is of itself sufficient to destroy all confidence in Mr. Brown's testimony. It proves a conviction in his own mind, that, had he communicated with *the only persons* who could possibly give satisfactory evidence with regard to these cases, it would have been adverse to his views. In the same spirit, accordingly, we find him bringing forward cases, in which he admits, that "the pustules were so small, and the inflammation so trifling, as to make the practitioner in attend-



ance suspect that he had not undergone the true form of the disease;" and others, in which the inoculator informed the parents, at the time, that the child was *not* properly vaccinated. Nay, he gives the names of respectable practitioners, as having performed that operation to their perfect satisfaction, on patients whom, to their knowledge, they never saw. "The oldest (of two children) was vaccinated by me," says Mr. Brown, "when four months old, in the arm; and the other by Mr. Keith, surgeon of the Berwickshire militia in both, about the same age. I recollect perfectly that the vesicle and areola were quite characteristic; and the mother describes, that the areola in both were equal to the size of half a crown; and that Mr. Keith saw the arm in its progress, and expressed himself satisfied of the child having passed through the disease." Now, the whole of this statement turns out to be incorrect; and the most charitable supposition is, that the mother imposed upon Mr. Brown, by saying what she saw was agreeable to him. "So far from having vaccinated the child in question," writes Mr. Keith, in a letter which we are authorised to quote, "I never, to my knowledge, *saw it*; but, on inquiry, find that it was vaccinated by *my hospital serjeant*, at the request of the mother, who supplied the hospital with milk; *the first and only time* of his ever attempting the operation. He saw it *but once* at the distance of eight days after; and at present only recollects, that the vesicle was *smaller* than he had been in the habit of seeing in those vaccinated by me." These examples fully develop the motives of Mr. Brown's conduct in carefully avoiding all communication with the gentlemen by whom these cases were said to be vaccinated; and explain why, in a pretended investigation of truths of such importance, he wilfully shut his eyes, and, when the object he ought to have drawn from nature was before him, chose to copy its reflection from a distorting mirror.

It should also be remembered, that, of those vaccinated gratuitously, either at public institutions, or by individual surgeons, a great number never return to show the progress of the vesicle; and yet, nothing was more common than for these people, in order to excuse their laziness and indifference, to report that the surgeons were well satisfied with the appearances, and had even taken matter from the arm. This, indeed, occurred so frequently, at the Vaccine Institution of Edinburgh, that



the managers were at last obliged to order, that each patient should deposit a pledge, to be forfeited, unless they attend regularly at the stated periods. This has had the best effects; though for a time it diminished the number of applicants.

Another prolific source of alleged failures, is the mistaking chicken-pox, and other eruptive distempers, for small-pox. That such a mistake has often been made, even by practitioners of much experience, cannot be doubted. The diagnosis of regular constitutional small-pox, is indeed abundantly easy; and if we were to refuse that name to every eruption which had not the nosological character, or did not correspond with the best systematic descriptions, we should hear very seldom of small-pox after vaccination. It would be foreign to our present purpose, to inquire into the nature of all the varieties and modifications of which small-pox is said to be susceptible. It is enough for us to prove, that, since the introduction of vaccination, eruptive diseases, of a very different nature, have been confounded with small-pox. Mr. Brown's cases are especially liable to objection on this account, because he has coupled them with a theory which sets all diagnosis at defiance. Mr. Brown contends, in direct contradiction to all analogy and observation, that before his period of security is elapsed, persons who have been vaccinated are liable to be partially affected by the small-pox; that at an early period it produces a slight rash; at a more advanced period, papulæ, which disappear without suppurating; afterwards pustules, which continue a few days; and at length complete small-pox. What opinion is to be formed of the professional knowledge or candour of a man, who records such cases as the following as instances of small-pox after vaccination? "The oldest fell sick on the Wednesday following; and, at the time I saw the other, was confined to bed, and had been so for two days. He was extremely sick, and affected with starting, sneezing, and other symptoms of eruptive fever. When I called, *three days after*, the sickness was gone, and *no pustular eruption followed*." p. 192. This is one of Mr. Brown's cases of natural small-pox. His inoculated small-pox is no less singular. Thus, in his twelfth case, "No constitutional symptoms could be detected; a slight heat appeared on the skin, but little or no alteration on the pulse." Next day, "from the report of Mr. M. and the maid, *she had*

"sneezed repeatedly, which they attributed to cold; and her appetite appeared impaired." The day after, "she had sneezed some the preceding night; only drank tea to break-fast, but eat no bread." Such statements are truly ridiculous; but we must refer our readers, for an able analysis of the whole series by the surgeons of the Vaccine Institution, to their report.

After such a refutation of Mr. Brown's statements, his hypothesis is scarcely worthy of any notice, in so far as it is founded on observation; and it is obviously contrary to all analogy, although he has attempted to bolster it up, by misstating the most universally received principles of pathological science. It is well known, that two general constitutional diseases cannot exist in the body at the same time. But it is equally well known, that an attack of such a disease, as soon as it is over, leaves the body as susceptible to the impressions of any other as it was before; nay, in many cases renders it much more so; since, in every elementary writer, we find debility from preceding disease enumerated among the causes predisposing the body to receive infection. Mr. Brown, however, has the merit of inventing a very different doctrine. "Indeed, there seems to be a general principle in the laws of the animal economy, that after it has been influenced by any power, it is, for some time, exempted not only from a repetition of its effects, but also from those of any other cause; and the distance seems in general to bear a proportion to the severity and extent of the power previously exerted." In another paragraph, he has still more luminously explained the principle upon which he supposes this exemption to depend. "These cases, and the whole phenomena and circumstances of vaccination, show, that there are just grounds for concluding, that a specific action may exist, *minus* or *negatively*, in the constitution; that therefore it would be improper, in the event of vaccination being found inadequate to maintain its antivariolous character, to reinoculate those cases which have previously undergone vaccination, before it was capable of producing a distinct constitutional effect." We really cannot persuade ourselves to reply to such arguments; but it is not a little singular, that his hypothesis should be directly contradicted by his own experience. Mr. Brown has vaccinated, in all, about



1200; of whom, upon his principles, and upon the supposition that he vaccinated nearly the same number every year while he continued the practice, 400 or 500 have now recovered their original susceptibility of small-pox infection, and 300 are liable to be affected by it in a mitigated form. But, of his alleged cases of failure in his own practice, *eight* only have occurred in the former class, and *ten* in the latter; so that, were we to credit his statements, the antivariolous influence of cow-pox would seem to increase, and to become almost doubled after five years. But it is not by Mr. Brown's experience that we wish any fact or opinion on the subject to rest; we must go to less suspicious authority. Every practitioner is acquainted with the numerous observations recorded in Willan and other writers on the subject. In addition to these, we have real satisfaction in quoting the general result of the experience of the surgeons of the Vaccine Institution at Edinburgh.

"With regard to the facts which have occurred in the practice of the surgeons of the Vaccine Institution, which tend to confirm or refute the doctrine of the mere temporary protection afforded by vaccination against the small-pox, the reporters beg leave to state, that the result of their experience is in strict conformity with that of Dr. Jenner, and the other advocates of vaccination. They have lately inoculated with small-pox, children who were vaccinated eight and nine years ago, and find that they completely resist the disease; they have not been able to produce on any of them more than a local inflammation, which disappeared in four or five days. They have, almost every year, visited numbers of children who were vaccinated during the first years of this institution; and this they have again done within these three months. In this investigation, they have found a great many of those who were vaccinated in the years 1801 and 1802, that is, seven and eight years since, who have been frequently and freely exposed, and especially within these last six months, to the contagion of the natural small-pox, by playing, sleeping, and otherwise mixing with children in all the different stages of that disease, without being infected." p. 32, 33.

The medical attendants of the Foundling Hospital of Dublin have also lately published an account of some very decisive and satisfactory experiments made in that institution, to dis-



prove the hypothesis, that the preventive powers of vaccination diminish in proportion to the distance of time from inoculation. Nine children, who had been vaccinated prior to July 1801, were inoculated with small-pox matter in July 1804, and exposed to the contagion in every possible way; and all of them resisted the infection. These nine children, together with ten others vaccinated between July 1801 and August 1802, were *again* submitted to small-pox inoculation on the 22d December 1809, (i. e. the first class upwards of *eight*, and the second upwards of *seven* years after the vaccination.) "In every instance," says Mr. Creighton the reporter, "the punctures in the arm of each child, from the third day, inflamed, and continued until the seventh, when the inflammation gradually subsided, as certified by Mr. Stewart, (surgeon-general,) and marked in a table, which, in another publication, will be more fully expressed;—which circumstance has proved the activity of the small-pox matter inserted, and which must have affected the constitution, was it in the least susceptible of the disease. Fourteen days (Jan. 4, 1810) have now elapsed: the inflammation of the punctures is entirely gone, and never was attended with the slightest *fever, sickness or eruption.*"

"In corroboration of the above facts," continues Mr. Creighton, "conducted with every degree of accuracy, and which cannot admit of the smallest doubt on the minds of those gentlemen who have witnessed them, and hereunto subscribed their names, I can safely assert, that I have submitted *upwards of five hundred infants* and children, vaccinated by me at this institution, and at the Dispensary for Infant Poor and Cow-Pox Inoculation, as established in the year 1800, to a like experiment, and *with the same result in every instance.*"

We ourselves had lately an opportunity of witnessing an equally satisfactory result in regard to the duration of the antivariolous effects of vaccination, from some trials made in a public hospital in this city, in which it was the practice to vaccinate, upon admission, every child which had not previously been vaccinated, or had the small-pox. A boy admitted in 1808, concerning whom no information was received, was erroneously supposed to have been vaccinated, and the operation was not repeated. In the beginning of October 1809, this boy,

although not more exposed to small-pox infection than an hundred other children living under the same roof with him, was seized with natural small-pox and had a very full crop of the distinct kind, which ran their course with perfect regularity. With *virus* taken from this boy on the 5th day of the eruption, seventeen children of the hospital were inoculated, who had all been vaccinated at former periods, varying from five months to *upwards of eight years*. The result of this experiment proved, *first*, that although a considerable degree of swelling and hardness, or even a distinct pustule on the arm with surrounding inflammation, may sometimes be produced by inoculation with variolous *virus*, yet the constitution is incapable of being affected with the small-pox: *secondly*, that these different effects, from a slight hardness to a distinct pustule with surrounding inflammation, are produced from circumstances altogether independent of the period intervening between the time of vaccination and the insertion of variolous virus: and, *lastly*, that the power of cow-pox, in protecting the constitution against the small-pox, is as complete at the end of eight years, as at the end of five months; and that, during *this* period at least, it is to be regarded as a perfect security.

There are even facts on record which prove that the antivariolous powers of cow-pox are permanent, or, at least, that they suffer no diminution in the course of upwards of half a century.

Farmer Jesty, according to the report of the Broad Street Vaccine Institution, visited London in 1805, and "afforded decisive evidence of his having vaccinated his wife and two sons in the year 1774, who were thereby rendered unsusceptible of the small-pox, as appears from the exposure of all the three parties to that disorder frequently, during the course of *thirty-one* years; and from the inoculation of the two sons for the small-pox fifteen years ago."

Dr. Jenner has recorded cases of persons who had been casually affected with the cow-pox, and had resisted small-pox upwards of fifty years; and, in a very excellent report of the Medical Faculty in Kiel, upon the cow-pox in the dutchies of Schleswig and Holstein, there is the remarkable case of a woman, then alive, who had the cow-pox when a year and a half old, and had remained secure against small-pox infection for



sixty years. In another case, the protection had then lasted 56, and in many, 40, 30, 20 years.

There is no fact, therefore, we conceive, relating to the animal economy, which can be considered as more decisively proved, than that the antivariolous powers of cow-pox do not decrease or wear out by length of time. At the same time, it must be admitted, that there are some apparently authenticated exceptions to the universality of its antivariolous influence. Within the circle of our own observation, none of these have occurred; and it is a very remarkable thing, that they are confined almost exclusively to the lower orders of society, and to the practice of certain individuals. Mr. Brown admits, and has attempted an explanation of this fact, in which we are desired to believe, that gentlemen, at the head of the profession, never hear of cases of small-pox after cow-pox, because the poor only are exposed to small-pox contagion; and that those who have most practice even amongst the poor, never hear of failures, because the poor never complain when disappointed and deceived, and never seek for assistance even in the most dangerous and loathsome maladies!

Upon the whole, we are satisfied that we concede more than is necessary, when we conclude our observations on the antivariolous powers of cow-pox in the terms of the Collegiate Report. "The security derived from vaccination, if not absolutely perfect, is as nearly so as can perhaps be expected from any human discovery; for, amongst several hundred thousand cases, with the results of which the College have been made acquainted, the number of alleged failures has been surprisingly small; so much so, as to form certainly no reasonable objection to the general adoption of vaccination; for it appears, that there are not near so many *failures* in a given number of vaccinated persons, as there are *deaths* in an equal number of persons inoculated for the small-pox."

The second general objection to cow-pox inoculation, is, that it produces new and unheard of diseases. This, we may observe, was first advanced as a conjecture, prior to all experience, and upon grounds purely hypothetical; though facts have since been referred to by these ill-auguring theorists. These theories, it is scarcely necessary to notice, as we are now in possession of sufficient experience to decide the question.



With regard, however, to the facts which have been referred to by the enemies of vaccination, nothing can be more vague and inconclusive. We have heard all the common cutaneous diseases, which uniformly attend on filth and poverty, attributed to the cow-pox. If in a scrophulous family, any symptom of that disease should appear in a child who had been vaccinated, no matter how long before, still the cow-pox is alone to blame for having engendered foul humours. Nay, if measles, or whooping-cough, or pleurisy, should be unusually fatal, even though the sufferers were never vaccinated, still the cow-pox is the cause of the mortality. Clamorous assertions of this kind, enforced by disgusting caricatures of mangy girls and oxfaced boys, have done more to prevent the universal adoption of vaccination, than any doubts of its efficacy. Of these, the most ridiculous, perhaps, is the frontispiece to a publication "by Ferdinand Smyth Stuart, Esq. physician, barrackmaster, and "great grandson to King Charles the Second," in which Dr. Jenner and his coadjutors, *cornuted* and *caudated*, are represented feeding a monster with baskets full of infants; while, to poor Dr. Thornton is assigned the final drudgery of shovelling them into a scavenger's cart, after being duly digested. The following is the poetical description by which this eloquent representation is illustrated.

"A mighty and horrible monster, with the horns of a bull, the hind hoofs of a horse, the jaws of the krakin, the teeth and claws of a tiger, the tail of a cow,—all the evils of Pandora's box in his belly,—plague, pestilence, leprosy, purple blotches, fetid ulcers, and filthy running sores covering his body,—and an atmosphere of accumulated disease, pain and death around him, has made his appearance in the world, and devours mankind—especially poor helpless infants,—not by scores only,—or hundreds, or thousands,—but by hundreds of thousands."

Dr. Mosely, again, has described, in technical style, a whole tribe of new cow-pox diseases; and Mr. Stuart has discovered a brutal degeneration of the human species.

"The cow-pox mange or farcy, cow-pox ulcers, with pus, green—green *as grass*, clearly demonstrating their bovine origin; cow-pox evil or abscess, cow-pox mortification, are nothing in comparison of the brutalization of the noblest work of the creation."—"Among the numerous shocking cases of cow-pox

which I have heard of, I know not if the most horrible of all has yet been published, viz. of a child at Peckham, who, after being inoculated with the cow-pox, had its former natural disposition absolutely changed to the *brutal*; so that it ran upon all fours like a BEAST, bellowing like a cow, and butting with its head like a bull. For my part," he adds with philosophical *scepticism*, "I can scarcely think it possible, having *had no time to ascertain the truth!*"

"O Mosely! thy books, nightly phantasies rousing,  
 Full oft make me quake for my heart's dearest treasures:  
 For fancy, in dreams, oft presents them all browsing  
 On commons, just like little Nebuchadnezzars.  
 There, nibbling at thistles, stand Jem, Joe and Mary;  
 On their foreheads, oh horrible! crumpled horns bud:  
 Here Tom with a tail, and poor William all hairy,  
 Reclin'd in a corner, are chewing their cud."

The diary of Dr. Barrackmaster Stuart's own child's illness and death, is truly humiliating, and excites a mixed emotion of ridicule and compassion. But as if the powers of language were not sufficient to excite our sympathy with his sufferings, and our indignation at the beastly disease which occasioned them, he has elucidated the history by a very amiable representation of Mrs. Stuart with the baby on her knee, the cradle on one side, and a bason of gruel on the other;—notwithstanding all which, we should have had no doubt that the poor babe's death was owing to *scrophula*, had it been of less than royal extraction.

But to be serious: The following appears to us to be a satisfactory answer to all this disgraceful clamour. Dr. Bateman, from the records of the Public Dispensary of London, has proved, that the proportion of cutaneous eruptions to all other diseases, was the same before the publication of Dr. Jenner's Inquiry, as in the sixth and seventh year of vaccination. And the Report of the London College states, "The testimonies before  
 "the College of Physicians are very decided in declaring, that  
 "vaccination does less mischief to the constitution, and less  
 "frequently gives rise to other diseases, than the small-pox  
 "either natural or inoculated. The College feel themselves  
 "called upon to state this strongly, because it has been object-  
 "ed to vaccination, that it produces new, unheard-of, and mon-  
 "strous diseases. Of such assertions, *no proofs have been pro-*



"*duced*; and after diligent inquiry, the College believe them to have been the inventions of designing, or the mistakes of ignorant men."

The last important objection which we shall notice, is, that vaccination has put an end to small-pox inoculation, without being so extensively adopted in its stead. In this objection may be traced the motives of many of the keenest opposers of the new practice; and it must be confessed, that its advocates have afforded some pretext for it, by their injudicious and unfounded complaints of want of patronage and encouragement. The truth is, however, that, when all the obstacles which vaccination has had to encounter, are considered, its progress must appear to have been inconceivably rapid. It has been adopted by millions who never would have submitted to variolation. For example, in this very city,\* gratuitous inoculation for small-pox had long been offered to the poor at the Public Dispensary, but altogether in vain; while, at the same useful charity, no less than 10,000 have been vaccinated since February 1801. This greater willingness on the part of the poor to inoculate for cow-pox than for small-pox, may be ascribed partly to their conviction of its utility and superior safety, and partly to its not being opposed by the mistaken but very powerful prejudice which prevails among the religious sectaries in this kingdom, that the wilful inoculation of any *disease* is an impious interference with the ordinances of the Almighty; while they do not consider the slight affection produced by vaccination as a disease. But while it has thus become much more general than small-pox inoculation ever was at home, the rapidity of its progress in the most remote corners of the earth, is altogether without example. It has been gratefully received by people of the most opposite races and religions, encouraged by governments of every description, and been the subject of publications in every written language. Manuals of vaccination, in the *Chinese* and *Polish* tongues, are now before us, illustrated with coloured figures. In our own colonies in the East-Indies, its success has been astonishing; and the numbers who have been vaccinated are such, that, in the settlements of Bombay, small-pox is said to be altogether exterminated. The reports of its progress in Ceylon are particularly interesting, on account of

\* Edinburgh.



its insular situation so nearly resembling our own. The following is the report made of it by a resident physician.

“The dreadful ravages which the small-pox usually committed in Ceylon, previous to the introduction of vaccination, must be in the recollection of every one; and it affords me infinite pleasure to observe, that, agreeably to the most certain information I have been enabled to procure, that destructive malady has not existed in any part of the British possessions on this island during the year 1808, except in the district of Galle, into which it was brought on the 31st of January by a Maldivian boat, last from Bengal. A large proportion of the crew of this boat died; and the disease was communicated by a fisherman, who visited it on its first arrival, to two or three inhabitants in the neighbourhood of Galle, but it spread no further; which must be attributed chiefly to the favourable influence of vaccination, which has been so extensively diffused in that and the other districts of the island.”

*Report 1809.*

We have a striking proof of the good effects of *general* vaccination. Contagion may be introduced; but it dies for want of susceptible subjects:—a firebrand may be applied; but there is no fuel to produce a conflagration. Even Old Spain was roused from its apathy by the obvious advantages of vaccination; and sent forth an expedition worthy of its better days, which circumnavigated the globe for the sole purpose of carrying to all its vast possessions, and to those of several other nations, the inestimable gift of vaccination: and in point of fact, it succeeded in disseminating it, not only through the boundless colonies of Spain, but through the vast Archipelago of the Visayan islands, and in establishing it wherever it touched in its progress.

So far as our information extends, therefore, we conceive there is no foundation whatever for this objection. Cow-pox, we verily believe, is far more generally resorted to than small-pox ever was; and the public, of course, must have great gain by the substitution.

So much for the objections: but we cannot allow the objectors to escape quite so easily. In a controversy as to matter of fact, where the witnesses contradict each other, it is absolutely necessary to ascertain, as far as possible, their relative credibil-

ity; and to settle our belief by comparing the number and value of opposite experiences. We have already seen, that these are decidedly, and almost infinitely, in favour of the advocates of vaccination. Still, however, the statements of their opponents may have been candid, and their opposition sincere; and the frequent occurrence of adverse facts would have perplexed us, and left doubts upon our minds with regard to the universal safety and efficacy of the practice. Fortunately, however, the conduct of the antivaccinists themselves has set our minds at ease. Their zeal has so far overstepped their prudence, and they have given such evident proofs of want of observation and candour, and have had recourse to such mean tricks and devices to frighten the timid and mislead the ignorant, as to deprive them of all credit with the well informed and judicious. What opinion must be entertained of the fairness or judgment of a man who could affix, on the walls of the most populous streets of London, posting-bills, displaying, in gigantic letters, "*Fatal Effects of Cow-Pox!*" with an earnest recommendation to heads of families to peruse the treatise in which they are stated,—who employs the drivers of errand-carts to distribute them indiscriminately to travellers upon the roads near London,—and could deliberately state, as one of his serious reasons for continuing the small-pox inoculation, that, "in the populous part of the metropolis, where the abundance of children exceeds the means of providing food and raiment for them, this pestilential disease is considered as a merciful provision on the part of Providence to lessen the burthen of a poor man's family!" Another device of the same gentleman, was the publication of a newspaper, for the exclusive purpose of attacking vaccination and its patrons with the lowest and most contemptible abuse.

But the most unjustifiable part of this gentleman's conduct is his wilful falsification of the report of the College of Physicians,—a public record widely distributed under the sanction of parliament. The few passages we have already quoted from it, will enable our readers to judge, whether it be in any respect warrantable to assert, "that the report of the College of Physicians allows the evidence, produced before the committee of the House of Commons, to be totally overthrown; that they allow there is no spurious cow-pox; and that failure, dis-



"order, and death, sometimes occur from some deviation in the genuine Jennerian cow-pox, which, after a precise period, fails in its security, and, if it does any thing, produces a new kind of eruption, tumour, or ulceration."—Mr. Stuart, however, improves upon this hint; and in his address to the British Parliament, thus expresses himself. "*Patres conscripti!* celebrated and illustrious senators of Britain, lay aside all prejudice, and receive, I entreat you, the following information with candour and attention, viz. That all the physicians, surgeons and apothecaries, most eminently distinguished for abilities and professional skill, *all to a man*, now acknowledge, that vaccination is not a certain preventive of the small-pox; and that it sometimes produces new, dangerous, and fatal diseases. These *truths* are at this time universally granted, and candidly acknowledged, by every intelligent medical gentleman; and this is all I contend for."\* Now, in these passages, there can be no misconception or mistake, to be accounted for by ignorance; and, therefore, we are afraid we must set them down as instances of wilful and unpardonable misrepresentation. Misquotation, indeed, seems to be a favourite figure with the antivaccinists; and with none more remarkably, than with their newly-enlisted Scottish auxiliary, who, we may remark by the way, wishes to be considered as the first writer, on that side of the question, entitled to any sort of attention; and treats all his predecessors altogether as cavalierly as his opponents. In proof of this gentleman's extreme looseness, and unfairness of quotation, we might refer to the greater part of his extracts from the public reports, and the writings of

\* The reader may take this further specimen of the eloquence and accurate reasoning of this scion of royalty.—"The Omnipotent God of Nature, the inconceivable Creator of all existence, has permitted *Evil*, *Buonaparté*, and *Vaccination* to exist,—to prosper,—and even triumph for a short space of time,—perhaps as the scourge and punishment of mankind for their sins, and for reasons no doubt the best, far beyond the powers of our very circumscribed and limited portion of penetration and knowledge to discover. But, are we to worship—to applaud—or even to submit to *Evil*,—to *Buonaparté*,—or to *Vaccination*,—because they have for some time been prosperous!—No!—Never let us degrade our honour—our virtue—or our consciences—by such servility:—let us contend against them, with all our exertions and might;—not doubting but we shall ultimately triumph, in a cause supported by *truth*, *humanity*, and *virtue*, and which therefore we well know *Heaven* itself must approve."



Dr. Jenner and Mr. Bryce; but we shall content ourselves with one example, in the case of Dr. Willan, whom he ingeniously contrives to quote as an authority for an opinion which he openly disavows, and that by the simple method of stating a sort of caution or exception to his general opinion, as the opinion itself.—“I shall, perhaps, be asked,” says Dr. W., “whether I think that the variolous eruptions, in all the cases adduced above, were the consequences of imperfect vaccination?” This is the question;—and here is the answer which is immediately subjoined; and of which Mr. Brown, when professing to quote both question and answer, has *omitted* the whole which we have put in italics. “*Vaccine inoculators were, at first, generally satisfied with any vesicular appearance, surrounded by inflammation; and even now, I believe, many practitioners would consider the specious irregular vesicle, described page 39, as a sufficient guarantee against the small-pox; not being aware how frequently it denotes a temporary incapacity to be affected by either the variolous or vaccine virus. I have had reason, on minute inquiry, to conclude, that, in a very great majority of the cases which occurred near London, the vaccination was imperfect. There is, however, great difficulty in obtaining clear and distinct information on the subject,*” &c. Willan, p. 73.—Brown, p. 317.

In the same manner, in quoting the admission of the London College, that cases of small-pox have occurred, after apparently perfect vaccination, he takes special care to leave out the word *apparently*; and, at last, makes that learned body admit, that such failures had occurred where there was “sufficient proof of the most perfect vaccination.” Dr. Willan’s treatise will also be searched in vain for any thing like the description of chicken-pox, which Mr. Brown has pretended to extract from it. When a person thus ventures to falsify public records to serve a particular purpose, it really is not easy to give implicit credit to statements made on his own authority, in opposition to general experience.

The established efficacy of vaccination as a preventive of small-pox, has given rise to various legislative projects for the utter extermination from this kingdom of that destructive pestilence; and it must be confessed, that our insular situation seems to give some encouragement to such a project.

Even prior to the discovery of vaccination, this had been strongly urged by Dr. Haygarth in this country, Scuderi in Sicily, and a whole tribe of enthusiasts in Germany. Insurmountable difficulties, however, appeared on every side; and nothing was ever attempted. The idea has been again revived, and certainly with greater plausibility, since the antivariolous powers of cow-pox have become known. Still, however, there are very strong, and perhaps insurmountable objections to every thing hitherto devised, or which perhaps may be devised for carrying it into effect. But, before we enter upon the discussion of these, we must notice some opinions of Dr. Adams, physician to the Small-Pox Hospital, and a pretended friend to vaccination; for if his opinion be true, viz. that cow-pox is identical with small-pox, or that they are but varieties of the same disease, it is plain that the diffusion of the one can never lead to the extirpation of the other; since, if there be any foundation for the opinion, that small-pox may be converted into cow-pox, it is impossible not to conclude, that cow-pox will, in many circumstances, degenerate into small-pox. Dr. Adams's arguments for their identity, are derived from the near resemblance of the most favourable kinds of small-pox to cow-pox, and "presumptive proofs deduced from the laws of all other morbid poisons, that the variolous and vaccine *is* the same." And he proceeds positively to state, that by "continuing with great caution to inoculate at the hospital from *pearl* small-pox, (the Doctor's hobbyhorse), we at last succeeded in procuring a succession of virus so nearly resembling the vaccine, that an universal suspicion prevailed among the parents, that they were deceived by the substitution of one for the other." The facts stated by Dr. Adams are certainly curious; but it appears that the practice said to have been followed by him was not necessary for their conversion, as has been satisfactorily proved by Mr. Bryce, (App. p. 70.); and, indeed, the facts and observations stated by that gentleman irresistibly suggest to the mind the mistake committed, at the commencement of the practice of vaccination, in the very same hospital, by Dr. Adams's celebrated predecessor. Dr. Woodville inoculated with variolous virus, when he believed he was using vaccine; and Dr. Adams seems to have reversed the matter.



His presumptive proofs proceed upon a notoriously erroneous assumption, that if a person be inoculated at the same time with the virus of two separate distempers, the one will remain unaltered till the other complete its progress, and will then take as many days to run its course as if it had not been inserted until the progress of the first was complete. The fact is, as stated by Mr. Bryce, "that both punctures will advance regularly, as if only one had been made during the period necessary for the local stage of these infections, and until the constitutional stage from one of them is excited, at which time, and not before, the progress of the other morbid poison, provided its local course be finished, will be arrested, until the first constitutional affection has disappeared." In the same manner, Dr. Adams misstates the fact when he asserts, that "if small-pox and cow-pox are inserted at the same time, in different parts of the same person, we find *no interruption whatever* in the progress of either. Both begin and go through their several courses with the same regularity, as if only one of them had been inserted in two different places." Now, Mr. Bryce's experiments prove incontestably, that as soon as a constitutional affection is produced, by small-pox for example, the further progress of the vaccine affection, if its local course be finished, is arrested until the variolous action has exhausted itself on the constitution; or it is altogether superseded according to circumstances. Dr. Adams is also wrong in his third position, that if a person be vaccinated for example, and in two or three days be inoculated in one place with variolous, and in another with cow-pox matter, the same consequences will follow as if both insertions had been of one matter. For the fact undoubtedly is, that, in such a case, the secondary vaccination will have its progress accelerated, and will arrive at maturity at the same time with the primary vaccination; while the small-pox pustule will *not* be accelerated, but proceed through its local stage in the usual manner. These facts prove, in the most decisive manner, that Dr. Adams's opinion is erroneous, even upon his own principles; and that small-pox and cow-pox are essentially different diseases, not convertible, in any circumstances, into each other.

Of all the plans for exterminating small-pox by means of cow-pox, which we have seen, Mr. Bryce's is the most detail-



ed, and will serve us as a text for the observations we have to offer. This plan embraces three several objects. 1st, To induce parents and others to have all children vaccinated before a certain age; 2d, To get correct lists of all those who have not been vaccinated; and, 3d, To put it in the power, and indeed in the way of all persons, to get the operation performed with skill and safety, by distributing corps of vaccinators up and down the country.

We shall not go into the various details with which Mr. Bryce has endeavoured to explain his project, and to obviate the objections he has anticipated. We shall only observe, with a view to the second branch of his plan, that however desirable such lists might be, we are afraid the procuring of them would be attended with greater difficulties than Mr. Bryce has foreseen. When we consider how inaccurately the registers of births and marriages and the bills of mortality are kept, and how many inconveniences, both personal and political, have arisen from this inaccuracy, we can only express our wishes, that vaccination may furnish a motive, sufficiently powerful, to lead to their simplification and correction; but we must confess, that we have no hopes of seeing any reform in this respect carried into effect. His corps of vaccinators, too, would never be tolerated, either by the public or by the profession; and would, at all events, soon degenerate into a scene of jobbing and intrigue.

It is the first part of Mr. Bryce's plan, however, which requires most attention, and about which there is likely to be the greatest difference of opinion. With regard to the aid which he seems to expect from the private patronage and exertions of men of influence and reputation, there cannot be a doubt, that it is the duty of every such man to instruct and enlighten the public with regard to the advantages to be derived from vaccination; and to remove the prejudices excited against it by those interested in the continuance of variolation. These however have hitherto had but a very inconsiderable effect; and it is well remarked by the College of Physicians,

"The lower orders of society can hardly be induced to adopt precautions against evils which may be at a distance; nor can it be expected from them, if these precautions are attended with expense. Unless, therefore, from the immediate

dread of epidemic small-pox, neither vaccination nor inoculation appear at any time to have been general; and when the cause of terror has passed by, the public have relapsed again into a state of indifference and apathy, and the salutary practice has come to a stand. *It is not easy to suggest a remedy for an evil so deeply imprinted in human nature.*" Bryce, App. p. 37.

It is this apathy or indifference which is the most powerful obstacle to the progress of vaccination; and we have considered its effects as the most serious objection to the practice. Should we, by means of it, succeed in banishing small-pox altogether from this island, or from large districts of it, there is some danger that vaccination would soon be very generally neglected, and that, so large a proportion of the people would be left susceptible of small-pox, that its effects, whenever it should chance to be imported, would be truly calamitous. It is on this account that we have heard very judicious persons dread the partial extinction of the small-pox. They would have it preserved, for the same reason that the clergyman would not have the devil killed, or that insurance offices rejoice in occasional fires. But the possible dangers of exterminating the small-pox are much too visionary to cause us to relax a moment in our efforts for that purpose; and, if we were to succeed in extirpating it in any one country, the danger of importing it would probably be much diminished, by the diminution of its source in other regions, and the encouragement which such an event would hold out to proceed against it with still greater vigour.

Are we, however, to use any other means than mere advice and example? Are we to resort to any measures of compulsion or restraint? Are we to have recourse to legislative measures? These are great political questions, in regard to which the present and late rulers of the state have expressed very different opinions; Mr. Perceval conceiving that more evil than good would result from any measure of coercion; and Lord H. Petty taking a different, and, we are inclined to think, a more correct view of the subject.

"Though I would not interfere," said his lordship, "with the freedom of an individual with regard to the mode of preserving his own health, yet I have no difficulty in saying that no individual has a right to conduct himself, even in the pursuit of preserving his own health according to the best of his judg-



ment, so as to endanger the health of a great portion of the community by spreading an infection, which is the case when individuals go abroad while they are under the process of inoculation under the old mode. This practice I understand to be increasing, and may be attended with dangerous effects. I know that in a country like this, where the inhabitants have been so accustomed to liberty in almost every thing, and in this practice among the rest, it must be difficult, and, without some infringement of liberty, perhaps impossible, to put an end altogether to this inconvenience. This, I am afraid, can hardly be done without some sort of compulsion;—and that is odious to the people of this country. But although compulsion be odious, while it calls on mankind to be active against their will, yet while it goes no farther than to forbid their doing that which is hurtful to others, I think that a state has, not only a right, but that it is its duty to enforce it. I would therefore say, that if persons will persist in following the old system of inoculation, they should be compelled to confine their practice within their own houses, and shall not be allowed to spread these ravages and this pest over the community at large." *Debates*, p. 74, 75.

The professional arguments for the restriction of small-pox inoculation are indeed very strong. Every person variolated becomes a centre of contagion, spreading disease and death around him. In addition to this obvious fact, and the instances quoted in various publications, we may state what took place in Weimar, both on account of its authenticity, and because, from its date, it cannot be suspected of exaggeration or colouring. The small-pox had not been seen in that city for about five years, when it became prevalent in the neighbouring villages. The duke, anxious for the safety of his children, wished to protect them by inoculation, but did not think himself entitled to take a step, however interesting to himself, which might endanger the lives of his subjects, without strongly warning them by advertisement, and inviting them to follow his example. Notwithstanding these laudable and truly paternal precautions, an epidemic was the consequence, which was distinctly traced to the ducal residence, and proved fatal to above fifty individuals in that small city. Even prior to the discovery of vaccination, in many countries small-pox inoculation was not per-



mitted except during the prevalence of an epidemic. But if such a restriction was at all tolerable then, it is surely much more so now, when it is in the power of every person to protect himself, and those under his care, without endangering the safety of others.

Mr. Highmore and Dr. Adams, *both of the Small-pox Hospital*, have argued in favour of small-pox inoculation on very absurd and untenable grounds. Dr. Highmore, for instance, is afraid, that if variolation be prevented, we shall not be able to test our vaccinated patients; and that the progress of vaccination will be impeded, if it be encouraged, as 'the flower which is forced into a too early maturity has neither strength nor fragrance comparable with that which blooms by fair and regular cultivation.' Dr. Adams' arguments are still more insidious, and equally futile. This gentleman apologizes to the public, for having so long delayed to offer, in print, his opinion on a subject so immediately connected with his engagements, and on which the public, he conceives, had a right to demand the result of his inquiries. But he was afraid of having his motives impeached; and there was a difficulty of finding any thing to oppose. At last, the letter to Mr. Perceval, in which Sir Edmund Carrington, late chief justice of Ceylon, shows, from our statute books, the legality of restraining every infectious disease, even small-pox, appeared to the physician of the small-pox hospital worthy of refutation; and a most singular attempt at refutation he has produced. As, on a former occasion, under the mask of a popular inquiry into vaccination, he had endeavoured to palm his *pearl-pock* upon the credulity of the people; so now, he endeavours to obscure the question relative to restricting inoculation, under the pretence of a general inquiry into the laws of epidemics. 'To defend small-pox inoculation,' says he, 'is only to repeat all that was said fifty years ago, and has been repeated ever since, till the last ten years. To admit that vaccination is a most important improvement, is equally superfluous. To say that this second improvement ought not by force to supersede the first, would only lead to those arguments by which small-pox inoculation was first defended; and to answer clamour and invective, requires a mind organized like those who use them.' To this we can only answer, that the case is totally altered within these ten years. Till then, we had only a choice

of evils, and we were not restricted in the only means offered to us of defending ourselves, lest in so doing we should injure our neighbours. But now that we possess a means of defence, equally useful to ourselves, and perfectly harmless to all around us, we are no longer entitled, either by reason or justice, to have recourse to the former. When Dr. Adams talks of a law restricting variolation, operating against the conscientious, without restraining the unprincipled or unfeeling, we must confess that we do not understand him. That none but the unfeeling would now have recourse to variolation, we might perhaps be disposed to allow; but that any such restriction would operate *against* the conscientious, so long as they have vaccination in their power, is what we will not admit. The great difficulty is, to prevail upon the mass of the people to use *any* preventive. With those, who have sufficient judgment to have recourse to one, the transition to a better is easy and natural. This was strongly exemplified in a fishing village in this vicinity. The first person in Newhaven, who had the courage and prudence to have his children inoculated with small-pox, was obliged to fly, as a monster, from the fury of his ignorant neighbours; and yet it was in that very village that vaccination first became general in Scotland,—and in that very man's family was it begun. Dr. Adams's whole chapter upon the recent plans for exterminating small-pox, is one of the most extraordinary pieces of reasoning we ever met with. We cannot exterminate small-pox, it seems, because constant and indestructible sources of contagion may be bought from every old-clothesman in Monmouth-street, and may be dug up in every grave! While these exist, the restriction of inoculation will not narrow its operations! We must not attempt to exterminate small-pox, because we do not know how to exterminate measles and scarlet fever; and because our ancestors never attempted any thing of the kind! And, lastly, restricting small-pox inoculation, and even forbidding the inoculation of outpatients at the hospitals, is compelling vaccination!

‘The discovery of vaccination is certainly a most invaluable acquisition; and those who are satisfied with it, do right to recommend it to the world. Happy for themselves and others, if they had been contented to recommend it by their example, and by the facility which the practice itself offers. If they go further than this, *there is only one step more* they can con-



‘scientifically take, that is, the forcing vaccination on *all*, under certain penalties. I know there are many men, whose intentions are perfect purity and benevolence, and who would start at such an imputation; but what else are we doing in prohibiting inoculation of small-pox, or even in refusing it to those who are unable to make a pecuniary return, or temporary seclusion? If they do not submit to vaccination, are they not without the chance of escaping six years, at most, many of them less than a month in the metropolis? Of escaping what? A disease which is said to destroy one sixth of the sufferers, besides maiming, blinding, and disfiguring many more. Do we know of any penalty heavier than an almost double decimation, and these additional torments?’

How far it would be prudent to forbid small-pox inoculation altogether, may admit of some doubt; but that the managers of the Small-pox Hospital acted rightly when they, at last, prohibited Dr. Adams from converting it into a source of pestilence, we conceive to be undeniably established by the statement made, without contradiction, in the house of commons, with regard to the effect of inoculating outpatients.

‘There is,’ said Mr. S. Bourne, ‘a very laudable institution in this country established for the inoculation of the small-pox. I understand it is the practice now to inoculate outpatients there, to the amount of 2000 a year; and that it is usual for these outpatients to resort twice a week to be inspected at this hospital by the surgeon. Now, it must be quite obvious, that this is a practice of the most dangerous nature; and that if we were to prescribe a mode of spreading the contagion, it would be difficult for human ingenuity to devise any thing better adapted for that purpose. No one would be more unwilling than myself to compel individuals to adopt any particular mode for the preservation of their health, because it is not in itself a proper subject of compulsion; but still I must say, that however reluctant I may be to use any restraint upon such a subject, some means should be taken to prevent the dissemination of this contagious malady. I think that the legislature would be as much justified in taking measures to prevent this evil, by restraint, as a man would be in snatching a firebrand out of the hands of a maniac, just as he was going to set fire to a city.’ *Debates*, p. 79.

*The Pathology of the Membrane of the Larynx and  
Bronchia.*

BY J. CHEYNE, M. D.

Edinburgh, 1809, 8vo. pp. 204.

From the Edinburgh Medical and Surgical Journal for the year 1809.

IN this volume we are presented with a republication of the author's former essay on croup, improved in its arrangement, and enlarged by some new and important observations; with cases of bronchial polypus, and of thickening and ulceration of the membrane of the larynx; and with observations on the epidemic peripneumony of children, and on peripneumonia notha.

Having with great precision described the ordinary form of the croupy paroxysm, Dr. Cheyne observes, that in the progress of the disease there are some remarkable changes, which may escape the notice of a hasty observer. These changes, however, distinguish the different stages of croup, and the degrees of danger to be apprehended.

"1st, There is the ringing croupy cough, (to which many children are liable upon taking cold, more particularly those who have had an attack of croup,) attended with little or no change in the breathing, or the sound of the voice.

"2d, The unusual shrill croupy cough, with difficult breathing; the necessary supply of air is with difficulty inspired from the constriction of the passage. The voice is altered, broken, both hoarse and puling. The difficult breathing in croup has been compared to the sound of air passing through thick muslin; it rather appears like the sound of a piston forced up a dry pump. This difficult breathing varies very considerably according to the degree of stricture. It is either like the sound to which I have likened it, dry and hissing, audible in different degrees; or when the swelling and spasm of the larynx is greater, it is crowing, and sometimes creaking and suffocative. Under this extremity of difficult breathing, children are said to have perished. Any person by voluntarily contracting less



or more the larynx may imitate every degree of the different breathing.

“ 3d, The cough and voice are stridulous; the respiration is difficult, laborious, creaking, sometimes suffocative, varying in the degree of difficulty and laboriousness.

“ 4th, The voice is whispering and low, the cough less frequent, and not audible at the opposite side of the room. There is the act of coughing without the sound; the respiration increasing in difficulty and quickness, laborious and interrupted.”

This account of the varieties of the state of the voice, breathing and cough, which take place in the progress of croup, is no less just than precise, and when connected with the very intelligent observations made by our author on these states, becomes highly important.

“ 1st, Is a state which is rather the forerunner of an alarming attack of croup. It is often without danger. It points out the children who, when exposed to the usual excitements, are most liable to croup. It would seem sometimes to be sympathetic.

“ 2d, When with the croupy cough the breathing continues difficult, the serious attack has commenced, and the child is in danger. From this state, it is true, I have known the child immediately recover, even when medicines of little activity were given; but generally, if no effort is made to save him, he will soon get beyond the reach of medicine. In this state, the child's skin is warm, his tongue is white, his pulse full and quick, and his countenance is much flushed; but it is still the flush of heat and fatigue. The usual mucous secretion is interrupted, he is timid and apprehensive, and, when advanced beyond infancy, he is willing to submit to any measures which may be thought necessary for his relief. His eye is heavy, watery, and bloodshot. In this state, the degree of danger is to be learnt from the state of the breathing.

“ 3d, This state shows the existence of the second stage of croup, that of effusion. Every person accustomed to the disease, knows how hopeless it generally is. The countenance is still flushed; but, in the flushing, we discover evidence of defective circulation. The lungs no longer purify the blood. There is a purple redness in the cheeks, eyes, and nails. The complexion is often mottled, or the flush on the cheeks is

circumscribed. The pulse is smaller, and very quick. There is sometimes an expectoration of mucus mixed with flakes of puriform matter. There is a sediment in the urine. The eyes are prominent and bloodshot; the pupil is dilated, and the iris has appeared to me pale. There is jactitation when the breathing is most violent, and lethargy when it is less so. During this stage, it is not unusual to find the child breathing with the least uneasiness, in postures which might be thought unfavourable to respiration.

"4th, This is the moribund state. The trachea is coated with effusion. The blood appears broken in the veins. The face is leaden, and the eye filmy. The extremities are cold and swollen. The muscular power is exhausted, and the child nearly insensible.

"In the first stage, the breathing is difficult; in the second it is both difficult and laborious. In the first stage, the affection of the breathing appears chiefly to arise from the state of the wind-pipe; in the second, as much, and perhaps more, from a disease of the whole pulmonary system. There is both heaving of the diaphragm and abdominal muscles, and a remarkable pulling down of the cartilages of the larynx at every inspiration."

In the next section, entitled, "an attempt to explain the pathology of croup;" having observed, that croup is the disease of infancy and youth, rarely if ever occurring after the age of puberty; that the larynx, before the occurrence of puberty, is but imperfectly developed, and the voice, therefore, weak and feminine; that, in a case of premature puberty the voice was grave and manly, while on the contrary, the voice of castrati never attains perfection, nor the larynx its full growth; the author concludes, that the predisposition to croup is connected with these peculiarities, and that the constitution becomes in a great measure secured from croup by the increase and vigour which the larynx and bronchial tubes acquire at puberty; or, that debility of the trachea is the predisposing cause of croup, and, that the greater degree of tone with which the trachea after puberty is endowed, enables it, in a great measure, to resist those excitements which would have operated on a less perfect organ.

That croup occurs as frequently in adults as in children,



with this difference, that adults possess the power of expectorating the lymphatic exudation before it becomes a solid membrane, as Michaelis and some others have supposed, Dr. Cheyne ventures to deny, on the following grounds.

"1st, In a disease chiefly of adults, in which also a membrane is found, the membrane is not expectorated until it acquires a density much greater than that of croup.

"2d, In the very rare cases of croup in adults, the power of expectoration does not appear greater than in children. By dissection it is proved insufficient to expel the membrane.

"3d, Inferring, from the effects of common catarrh in changing the sound of the voice at all the stages of life, the cough which exists before the membrane is formed should be equally characteristic of a diseased state of the organ in adults as in children.

"4th, In adults, in cases of diseased larynx, when the membrane of the trachea is coated with exudation, there are symptoms corresponding to those of croup in children. The voice is stridulous and whispering, and the cough suffocative, and the expectoration is exceedingly difficult."

Inflammation of the mucous membrane of the larynx and trachea, consequent spasm of the muscles, exudation of lymph, formation of membrane, with altered mucous secretion, preventing the purification of the blood, producing sensorial debility, and finally suffocation and death, make up the pathology of the disease. In croup there is increased action, effusion, laborious respiration, circulation of blood, with venous colour, sensorial debility, and death. "This," says our author, "is the course of the disease."

In treating of the diagnosis, it appears to us, that Dr. Cheyne is either too nice and fastidious, or that he is establishing distinctions without differences. If the cough, voice, and mode of breathing be those of croup, and if a membrane be actually found on dissection after death, although sloughs may have been observed on the uvula and tonsils, or although the paroxysm may have supervened to, or been complicated with scarlatina, with measles, or with small-pox, still it is croup, not indeed pure and idiopathic, but, though complicated, still croup, still inflammation of the membrane of the larynx and trachea, exudation, and formation of membrane, giving occa-

sion to the same series of symptoms, which distinguish idiopathic croup. Yet, says our author, "*whatever be the symptoms, when the fauces are sloughy, I am inclined to think that the disease is not to be considered as a case of pure croup; and I would not admit as cases of croup, any complications of croupy symptoms with other diseases, unless these diseases, after weakening the membrane of the larynx and bronchia, are superseded by croup.* To us it seems, that this is a very unnecessary refinement, and that it is better to say at once, that croup, though commonly idiopathic and uncomplicated, may, like other phlegmasiæ, like cynanche, ophthalmia, or pneumonia, for example, occur symptomatically in the course of other diseases, particularly of the order exanthemata.

The operation of bronchotomy has been proposed to be attempted in the second stage of croup, seemingly, however, without a just conception of the true pathology of the disease. The inexpediency of this operation is, at any rate, very clearly made out by our author. From his observations, it appears, that it cannot be necessary for the purpose of admitting air into the trachea; for in those who have died of the disease, he has found a pervious canal, of two-eighths of an inch in diameter, and through a tube of such diameter, even an adult can support respiration for a considerable time. It is equally unfitted for the removal of the membrane; for, from its extent, variable tenacity, and adhesions, this is, in almost every case, totally impracticable; and even, could the whole membrane be removed, still the function of respiration would be but little improved, the ramifications of the trachea and bronchial cells remaining obstructed.

The observations which follow on the treatment of the croup, though short, are deserving of every attention, as being the result of considerable experience.

It is, with great truth, observed, that the first, or inflammatory stage of croup, generally lasts only eight or ten hours; and that in a disease so acute our means should be well adapted to the end. There appears no time for alterative means; and, therefore, blood-letting is a remedy hardly to be dispensed with at this time. Generally, he prefers bleeding from the jugular vein. When bleeding is performed at the commencement of the attack, the relief is often immediate; and he has



scarcely believed that he saw the same child breathing easily, who, ten minutes before, lay gasping and convulsed.

But, although he does not appear to have yet acquired that confidence in the use of calomel in croup that some physicians place in it, we observe, that he speaks more cautiously, and, upon the whole, more favourably of its powers than he did in his former essay.

When the attack is alarming, he has never, indeed, in the first stage, given calomel a fair trial, having never trusted to it alone. In the second stage, nothing can be expected from it; and we are, therefore, not surprised to learn, that, though freely used in a variety of cases, all of them terminated fatally.

He observes, however, of this remedy, that "when a serious attack is apprehended, as in the croupy cough, with febrile heat, experience enables me to say, that a free use of calomel is serviceable." He thinks it applicable also to cases which arise during the currency of other diseases; to cases of the disease extending beyond the usual period, of a nature perhaps approaching to bronchial polypus; to cases where there are remarkable intermissions; and to less urgent cases, and such as occur in debilitated and scrofulous children.

Emetics are useful in every stage of croup, and are much employed and recommended by him; and, in the second stage, are considered as the chief agents.

His general plan of treatment is thus summed up:

"The means to be used when an attack of croup is apprehended, are an emetic, a bath of between 90 and 100 Fahrenheit, a dose of jalap and calomel, and dilution.

"When the first stage is formed, we have recourse to an emetic, the bath, a mercurial purge, venesection, a blister over the sternum, calomel in doses of one, two, or three grains, every hour, diluents, the antiphlogistic regimen.

"In the second stage, emetics are the chief agents."—"Half a grain of tartarized antimony, dissolved in a table spoonful of water, is to be given to a child two or three years old every quarter of an hour, until sickness and vomiting are produced; and, in two hours after the last act of vomiting, the same process is to be recommenced, and so repeated while the strength admits."—"Blisters are applied, or the breast rubbed with oil and tincture of cantharides. Medicated vapours in diseases of

the membrane of the bronchia have been highly recommended; in croup, they have disappointed my expectations. When the strength begins to flag, we use all the common cordials. One child gradually emerged from the second stage of croup, after having been in the greatest danger for three weeks; he was supported chiefly by wine and burnt brandy. As the stools are so much disordered, the state of the bowels must be attended to. The antimonial solution generally acts sufficiently on the bowels."

"There is but one indication in the first stage of croup. All our endeavours tend to moderate the increased action which prevails all over the mucous membrane.

"The bleeding, blistering, emetics, purges, and the bath, are all meant to reduce the power of the arteries, either directly or sympathetically. The mercury, however, by establishing its own influence, may be supposed to supplant that of the disease.

"In the second stage, we have various objects: 1st, We have to reduce and correct the increased action of the mucous membrane; 2dly, To promote the expectoration of the adventitious membrane and the effused fluid; 3dly, To support the patient's strength."

Of late, it has been common to distinguish two distinct cases of croup; the true and spurious, or spasmodic. The observations of our author have not enabled him to perceive the justice of this distinction. In some cases, indeed, the croupy voice and breathing have recurred in paroxysms; and when these ceased, there were intervals of natural breathing. But these, and the cases described by Dr. Ferriar and others, he seems to think differ in degree rather than in kind from true inflammatory croup. The spurious croup arises from the same exciting causes, prevails during the same state of the weather, attacks the same families and the same children who are liable to true croup, invades at the same time of night, and degenerates sometimes into inflammatory croup. It requires, indeed, a less active treatment than the inflammatory or true active croup; but the general plan is the same for both. If the symptoms be moderate, blood-letting may be omitted, and the cure trusted to emetics and calomel. But it is the state of the respiration which must guide us in making this selection.



"Until the advocates for the separate existence of spasmodic croup more fully assign and establish their grounds of belief, I am convinced, that it will be for the benefit of the patient that we should act as if there were but one kind of croup, and of every mode of this disease, previous to the unequivocal formation of the second stage, that we allow the treatment to be regulated by the state of the respiration."

"The symptoms induced by thickening of the membrane of the larynx, are pain in the larynx, not very acute, unless on pressure; some degree of fulness externally; a change in the sound of the voice; difficult and severe crowing inspiration, but slow rather than quick; an altered, sometimes stridulous voice; fits of suffocative coughing, and all those general symptoms which arise from obstructed circulation in the lungs."

Of this fatal complaint, a few very interesting cases are given; and the pathological observations of the writer are highly worthy of the reader's attention. The disease appears to be a far more common one than might be expected from the silence of most collectors.

Ulceration of the larynx, with similar symptoms, and the same fatal event, occasionally supervenes to measles and small-pox.

The next observations are on the epidemic peripneumony of children, a very common disease, and of which we have here an excellent description.

The remedies recommended by Dr. Cheyne are those used in other inflammatory affections of the lungs; vomiting, purging, bleeding, blisters, the tepid bath and diluents, repeated doses of calomel, small doses of opium after the evacuations.

We have seen a very different method of treatment recommended by Dr. James Hamilton.

The observations on peripneumonia notha are somewhat desultory, and intended to show, that other diseases of the bronchial membrane, epidemic peripneumony, and catarrh, sometimes terminate in that disease. All the diseases, he observes, which terminate in peripneumonia notha, are attended with increased action of the mucous membrane of the bronchia. Peripneumonia notha is generally a combination of this action, with the effects of a relaxed habit and weakened organ.

To conclude, we do not think Dr. Cheyne had much occasion to apologize to the purchasers of his essay on croup; for, from the numerous additions made to this, the improved arrangement of the whole, and the new and interesting pathological observations on other diseases of the bronchial membrane and larynx, this must rather be considered as a new book than as an improved edition of his former essay. Our limits have not allowed us to advert to the numerous cases, dissections, and engravings, which illustrate the text.



## ORIGINAL PAPERS.

*Observations on the external use of Oil of Turpentine  
combined with Cantharides.*

BY JOSEPH HARTSHORNE, M. D.

THE efficacy of different irritating applications to the surface of the body, in subduing internal as well as external disease, has been so often demonstrated, and their use has become so familiar to all classes of practitioners, that I fear it may be deemed presumptuous to offer any new ideas on so trite a subject. Believing however, that an attempt to call the attention of enlightened physicians to this part of our science may eventually produce some improvement in practice, I venture to submit to their consideration a few desultory remarks on the external use of the Spirit of Turpentine and Cantharides.

The power which <sup>the former</sup> ~~this substance~~ possesses of changing the excitement of a burnt part from a diseased to a healthy state is now generally known. May I be permitted to ask why the use of this essential oil should be restricted to one disease of the skin? If fire produce an increase of action which has a tendency to terminate in the death of the injured part, has not the excessive excitement produced by other destructive agents the same tendency?

The disease excited by fire passes through its different stages with such rapidity that we can seldom err by a too early application of stimulants. The virus of the small-pox affects the whole system, but its principal force is exerted upon the skin, producing in that organ inflammation and ulceration, which sometimes terminate in gangrene: but as this poison operates more slowly than fire, that state of exhaustion which requires the exhibition of stimulants does not occur until near the close of the disease. In the typhus stage of the most malignant cases of variolous fever, the practice most generally approved is to give stimulants internally, and to apply epispastics to the extremities. Would not the application of the spirit of turpentine diluted with common oil, to the whole surface of the body be attended with more success?

On the 28th of May, 1809, John Mitchell, aged about forty years, was placed under my care on the third day after the eruption of casual small-pox. This was the most violent case I had ever seen. During the inflammatory stage venesection, cathartics and calomel in small doses were prescribed. It soon became necessary to change this plan of treatment, as the disease rapidly progressed to that stage of debility from which few recover. On the sixth of June the skin was entirely covered with large vesicles; pulse extremely feeble; extremities cold, and the tongue dry and hard. Peruvian bark, in the forms of tincture and decoction, was given in small doses frequently repeated. Epispastics were applied to the thighs and arms.

June 7. The epispastics produced no effect; sinapisms were now applied to the legs; they excited not the smallest degree of pain or inflammation; volatile alkali, ether, laudanum and brandy were freely administered.

June 8. The stimulating treatment was industriously continued during the whole of the last twenty-four hours; no advantage was derived from it. Respiration at this time was exceedingly laborious; pulse at the wrist scarcely perceptible, and his breath not warmer than the surrounding atmosphere. The extremities were anointed with a liniment composed of one part of spirit of turpentine and two parts of basilicon. Three hours after this liniment had been used the skin became warm, and the temperature of his breath was raised considerably. Brandy, tincture of bark, &c. were continued.

On the 9th, respiration was natural, and the skin became warm as in health. This was the first time during the whole course of his illness that the patient expressed a desire for food. He was ordered a teacupful of chicken water every two hours. The nurse however gave him a large bowlful of rich soup. Four hours afterwards he died.

By reflecting on this case, I was induced to believe that the action of cantharides on debilitated subjects might be rendered more certain by combining them with essential oils.

My friend, Dr. Calhoun, at that time resident physician of the Pennsylvania Hospital, instituted at my request a series of experiments, with a view of ascertaining the properties of a



preparation made by boiling for three hours one ounce of powdered flies in eight ounces of spirit of turpentine.\*

This preparation when filtered is much more active than cantharides alone. Considerable pain is excited in less than half an hour after its application, and in about four hours vesication is generally produced. In a case of typhus fever under my care last winter, epispastics in the usual mode were applied to the wrists, and pieces of linen dipped in the decoction of cantharides were confined to the ancles. The pain created by these different applications was nearly equal; the usual burning sensation which accompanies the action of cantharides commenced earlier and ceased sooner in the parts to which the decoction had been applied. This patient received great benefit from frictions with the same preparation of cantharides diluted with linseed oil. A slight degree of inflammation and an agreeable warmth were the immediate effects of its application. The ~~use~~ of the frictions <sup>were</sup> continued several weeks, and were occasionally rendered more stimulating whenever the pulse seemed to flag, or the temperature of the skin to fall below the natural standard. Cordial drinks and invigorating aliment soon restored the patient to perfect health.

On the 23d of December, 1809, my friend Dr. Parrish requested me to visit with him Caleb Hatheway, aged about ten years, who laboured under typhus of a most malignant aspect. Epispastics and sinapisms had been very extensively used on the 22d and 23d; Madeira wine was at the same time freely exhibited. A feeble intermitting pulse, a discharge of dark-coloured blood from the gums, an entire insensibility to surrounding objects, an accumulation of heat about the head, and a remarkable coldness of the extremities, were symptoms that left very little room to hope for the recovery of our patient. We agreed, however, in opinion, that the use of stimulants to the skin was clearly indicated; and as we had already experienced in this case the inefficacy of mustard and cantharides in the common mode of preparing them, we determined

\* As the oil of turpentine boils at a low temperature, very little fire is necessary in making the decoctions. A Florence flask and sand both may be used; or if the latter should not be at hand, a chafingdish with a few coals covered with ashes will answer the same purpose.

to use frictions upon the lower extremities with the decoction of cantharides diluted with olive oil. No other change was made in the treatment. About three hours after the frictions had been used, the feet and hands resumed their natural warmth. On the morning of the 24th the patient gave rational answers to any questions proposed to him. The frictions had inflamed the legs and thighs considerably, and in some places vesications appeared.

In the course of the day, whenever the extremities became cold, the preparation of cantharides still more diluted was applied to them. The infusion and tincture of bark in ten or twelve days completed the cure.

The relief afforded by vesicatories in diseases of the stomach and bowels is often transient, and few patients are willing to submit to their repeated application. Their inefficacy is owing in many instances to the smallness of surface to which they are applied. A more beneficial and more durable impression may be made by exciting a gentle action upon the whole external surface, than by the most intensely irritating applications to a small surface.

In several cases of protracted cholera infantum, in which internal medicines appeared to aggravate the disease, every unpleasant symptom has gradually yielded to the rubifacient plan of treatment. In the typhus, which so often succeeds bilious fever, there is no external remedy with which I am acquainted that is so powerful as the decoction of cantharides in exciting a general increase of action in the vessels of the skin. The stomach in this disease is sometimes so torpid that the purest alkohol is inert as simple water; but after the arterial system has been roused to activity by the extensive use of irritating frictions, the excitability of the intestinal canal is restored, and internal medicines may then be exhibited with great advantage.

In alleviating the pain of departing life, the last melancholy duty of our profession, experience enables me to assert, that anointing the body with warm stimulating oils is sometimes more useful than opium. At that interesting period of existence, when the patient and his friends, after abandoning all hope of recovery, still solicit the medical attendants for relief, opium, judiciously administered, will frequently succeed in



"smoothing the passage out of life:" but how often does it happen, that in consequence of exhibiting this medicine, the power of expressing pain is lost in stupor or delirium.

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*Case of Strangulated Hernia, with some Observations on the Treatment of mortified Omentum.*

BY JOSEPH PARRISH, M. D.

ON the third day of the third month (*March*) last my immediate attendance in consultation was requested by my friend Dr. Samuel Tucker, of Burlington, N. J.

The patient was a farmer, of middle age, who led a laborious life, was of temperate habits, and the parent of six children.

He had been occasionally afflicted with scrotal hernia for fifteen years, but had never worn a truss, or disclosed his situation to any person. When it proved troublesome, he had been in the practice of reducing it without difficulty.

On the morning of the 28th of second month, while in the act of lifting a heavy log, a portion of the abdominal contents was suddenly protruded through the ring, and became strangulated. He had an alvine discharge immediately after.

From the period that Dr. Tucker first saw him, until I was called, he had diligently resorted to the most approved plans of reduction; viz., taxis, venesection, applications of ice to the tumor, tobacco injections, warm bath, &c. &c. but all without effect.

When I saw him, his chief distress appeared to arise from vomiting and hiccough; the latter always occurred after drinking. His pulse was remarkably tranquil, tongue moist, and but slightly furred; no tension or tenderness in the abdomen; and it was not until the latter part of that day that he was sensible of darting pains, which occasionally extended from the stricured part towards the abdominal cavity. The tumour was of moderate size, and rather firm to the touch.

As Dr. Tucker had decided on the necessity of the operation previously to sending for me, it only remained for us to obtain the patient's consent; but this was rather difficult, for he was very indecisive, sometimes partly consenting, and then refu-

sing. It was night when I visited him, and under all circumstances, it appeared as if nothing could be done until daylight. The mildness of his symptoms reconciled us more readily to this conclusion. He had slept well the preceding night without an anodyne. A small enema containing about two drachms of Thebaic tincture was given him, and directions were left to repeat it in an hour if the patient should be restless.

Dr. Tucker and one of his friends saw him about sunrise. He walked from his bedchamber into the common room, handed chairs, invited them to sit down, said he had passed a good night, and in fact had quite abandoned the idea of having any operation performed.

I saw him soon after; and we again endeavoured to explain to him the extreme danger of his situation, and he at last consented to the operation.

One grain of opium was exhibited, and he was placed on a table. An incision was made through the skin sufficiently large to allow a free examination of the parts about the neck of the hernial sac. While carefully dissecting through the integuments, three arteries were divided and secured by ligatures; the largest was found running directly across and just below the neck of the tumour. Several tendinous stricturing bands were brought into view and divided; but after every apparent external cause of stricture was removed, the prolapsed parts were still irreducible. The incision was extended along the scrotum nearly to the bottom of the tumour, and the hernial sac was laid open. A fluid of a bloody colour issued from it.

It was now evident that the chief seat of the stricture was in the neck of the sac; it was contracted firmly round the protruded parts. The tip of my finger was introduced as a director for the blunt pointed bistoury, with which it was readily divided.

Its contents consisted chiefly of omentum, of which there was a much larger portion than would have been imagined from the size of the tumour. I think there were nearly eight ounces. Along with this, and lying in the very centre of the omentum, was a portion of intestine, which passed about an inch and a half beyond the stricture. It appeared nearly na-



tural, but the omentum was in a very different state; a considerable part of it was sphacelated, particularly its exterior surface, which was quite black, and its vessels were greatly distended with coagulated blood. Some other portions were of a light mahogany colour, and were brittle when pressed between the fingers. The central part of the mass was chiefly natural.

The intestine was speedily reduced; but for reasons to be hereafter assigned, the omentum was left in the wound. Three sutures were used in uniting the lower part of it, so as to close it as nearly as was practicable without compressing the omentum. Soft and light dressings were applied over the whole.

The patient appeared faint about the close of the operation; he was presented with a little wine and water, but it was rejected by the stomach. He was now placed in bed, and soon sunk into an easy and profound sleep. He was in this state when I left him, about an hour and a half after the operation. His pulse was fuller, and yet free from tension.

A very light diet of chicken liquor, barley water, &c. was directed. Also, *Ol: Ricini*, half an ounce every two hours until it operated.

In a letter from Dr. Tucker, he reports:

"Our patient rested well the night after the operation. He took four or five spoonfuls of castor oil; it began to operate at four o'clock in the morning, and relieved his bowels five or six times. I left directions in the evening, that if the oil operated excessively, it should be checked, by taking five drops of laudanum. His wife gave him the laudanum about noon the next day.

"*Monday evening, 5th.* His bowels had not been moved since noon. I directed him to take the oil again until it operated. No fever, pulse 75.

"*Tuesday morning, 6th.* Rested well last night—no fever—no pain—pulse 75. Castor oil has operated twice.

"*Evening*—the same.

"*Wednesday morning.* Did not sleep well last night. When disposed to sleep, started, which gave him some pain, and prevented its recurrence for some time. He does not, however, appear to be worse. No fever, pulse 75—takes chicken broth, barley water, &c."

On the 11th of the month I visited him in company with Dr. Tucker. He was then perfectly free from pain and fever; no tension or tenderness in the abdomen; union, by the first intention, had taken place in the part of the wound approximated by sutures; and the living omentum situated within the wound, and in contact with its edges, appeared to have adhered to them, and to have closed the cavity of the abdomen.

Subsequent information from Dr. Tucker enables me to state, that on the 18th the last portion of unsound omentum sloughed away, leaving the living part divided into two distinct portions, suspended from the wound by two necks.

On the 21st a ligature was applied to one half of the omentum, in the manner recommended by Hey; viz., rather slight at first, and increasing gradually as the patient could bear it. On the 25th it was perfectly black and flaccid, and was removed by scissors; on the 26th another ligature was applied to the remainder, and at this time the wound had cicatrized, except where the tumour was suspended from it. In both cases there was a considerable oozing of blood after the omentum became black, but surrounding the part with lint put a stop to it. On the 5th day from the application of the last ligature the tumour was removed.

In about five weeks after the operation he began to walk about the house; and in eight he resumed his agricultural avocations, and ploughed a large field for the reception of Indian corn. Since this period he has enjoyed very excellent health, and wears a truss to guard him from future danger.

As the proper way of disposing of mortified omentum remains unsettled among surgeons, I shall assign the reasons which induced us to adopt the practice now stated.

It must be admitted by all that difficulties pressed on every side. We were afraid to cut off such a large mass, and return the living part into the abdomen, because, from the account of that eminent surgeon, W. Hey, of Leeds, such practice may involve the patient in extreme danger from subsequent hemorrhage; and he gives it as the result of his experience, that he never would do it again, unless he placed ligatures on every bleeding vessel; but instead of this, it appears that he suffered the omentum to remain in the wound.

To pass ligatures round the bleeding vessels appeared



hazardous, for they would probably have been numerous, and we were apprehensive that so many extraneous bodies in the cavity of the abdomen would increase the danger of peritoneal inflammation.

To include the whole mass in one ligature would have been simply to renew the strangulation we had been engaged in removing; and the annals of surgery are not deficient in histories of the fatality of such practice.

Under these impressions, we were united in imitating the plan recommended by the candid and experienced Hey, and the event has justified our most sanguine expectations.

I am aware that objections have been offered to this practice, and confess myself not a little surprised to find it totally rejected in the late valuable Surgical Dictionary, by Samuel Cooper, who appears to found his opinion on a most excellent treatise on hernia, by William Lawrence, which I have perused with pleasure and instruction. And as the object of these writers appears to be the attainment of truth, they will, no doubt, excuse a diffident attempt to examine the arguments which they offer on the occasion. In Lawrence, page 180, et seq., we are informed,

"Some surgeons have recommended that the omentum should be left in the wound, particularly in an old hernia where the parts have been long down.

"Cases are recorded, which show the safety of this practice, and which prove that granulations extend over the omentum, and that a firm cicatrix ensues.\* This practice, which I cannot speak of from my own observation, does not appear to me to deserve recommendation. It is attended with no particular advantage, and certainly exposes the patient to the possibility of ill consequences. The omentum left in the wound must be liable to injury, inflammation, or disease; and hence arises a source of danger to the patient. Unnatural adhesions, formed by this part, have greatly impaired the functions of the stomach. Cases are recorded where the unfortunate patient has never been able to take more than a certain quantity of food, without bringing on instant vomiting; and even where it has

\* Hey, p. 180, et seq. Chopart and Dessault state, that when the omentum is irreducible merely from its bulk, they leave it in the wound, and it gradually retires into the abdomen. *Traité des Mal. Chir.* tom. II. p. 269.

been necessary for all the meals to be taken in the recumbent position, with the trunk curved and the thighs bent.† To avoid the possibility of such afflicting consequences, we should, after removing any diseased portion, carefully replace the sound part of the omentum in the abdominal cavity, that no obstacle may exist to its regaining that situation, in which its connexions with the stomach and colon would naturally place it."

Now in reply to the objection that the omentum is liable to injury, &c. I have only to observe, that in the present instance no difficulty occurred about keeping it from injury, and no inflammation supervened. The same appears to have been the experience of Hey with his patient.

As relates to the dreadful train of symptoms produced by adhesions of the omentum, as mentioned by Gunz, I must again express my surprise, that these respectable surgeons should be under the necessity of quoting foreign writers, instead of giving us their own experience, or that of some of their brethren at home; for, although I do not feel disposed to doubt the authority, yet such cases must be very uncommon, or they would have occurred in the practice of some of the most celebrated surgeons of Great Britain.

Thus we read of Percival Pott operating on a large irreducible epiplocele that was troublesome merely on account of its bulk. Now we must conclude, that if the dreadful symptoms attendant on adhesion had occurred, they could not have escaped his notice, but would have been offered as a reason for performing so hazardous an operation.

Astley Cooper, a later writer, could not have laboured under apprehensions of this kind; for in his valuable work on hernia he mentions cases where, after cutting off the omentum, he left it in the mouth of the hernial sac, hoping that it would adhere to it, obliterate the opening, and thus produce a radical cure.

I confess that my anxiety was excited on account of danger from adhesion; but the issue of the case has proved it groundless. And when we recollect the admirable power of the system to adapt itself to exigencies whereto it is subjected,

† Gunz. *Obs. Anat. Chir. de Herniis. Memoires de l'Academie de Chirurgie*, tom. III. p. 406."



it is not surprising, that either by an elongation of the parts, or in some extraordinary manner, anticipated danger is eluded.

With respect to placing ligatures on the bleeding vessels, it may be proper to remark, that I do not find one single case of the kind recorded in Cooper on Hernia: The portions cut away were small, and ligatures were not requisite. But in the second volume of Transactions of a Society for the improvement of Medical and Chirurgical Knowledge, a case is related by Everard Home, where two ligatures were placed on the divided omentum; the intestine involved in the stricture was found in a diseased state; the patient died on the tenth day from the operation, and her death was referred to enteritis previously induced.

Now it appears reasonable to conclude, that a small quantity of omentum may be cut off with impunity, while the danger from hemorrhage would be great when a larger portion is subjected to the same treatment; because the nearer to its origin, the greater the size of the vessels. In the present instance, they were large and numerous.

Upon the whole, it seems not uncandid to conclude, that as Lawrence informs us, that on this particular point he cannot speak "from his own observation," he does not pursue the right plan of treating a practical subject, who places his mere opinion in opposition to the experience of a surgeon justly entitled to great eminence, who assures us that he has had "three patients of the kind, all of whom recovered." Hey, p. 131.

It is the more extraordinary, because the same writer, when treating of the management of mortified intestine, very judiciously condemns the practice of cutting away that portion of the bowel; observing, that "almost all the numerous instances of recovery from mortified hernia, recorded in the annals of surgery, took place where the surgeon was contented to remain a quiet spectator of the process, without interfering with any artificial means of uniting the divided intestine," &c. Lawrence on Hernia, p. 202. And immediately after, in page 203, he quotes the opinions of J. L. Petit and Richter, "two celebrated men, whose acknowledged abilities and extensive experience entitle them to the greatest attention." The quotation from the former is so very appropriate, that I shall venture to transcribe it.

“ Cette observation, et quelques autres, que j’ai rapportées ci-dessus, prouvent bien, que les guerisons, qui paroissent miraculeuses, sont dûes a la nature plus qu’a l’art.

“ Heureux les malades, qui tombent entre les mains des chirurgiens bien convaincus de cette verité; ceux-ci s’attachent seulement à éloigner tout ce qu’ils croiront pouvoir troubler au interrompre la nature dans ses fonctions, et n’en auront pas moins de gloire.” *Traité des Mal. Chir.* tom. ii. p. 403, 404.

If, then, we are strictly enjoined to avoid interfering with nature in the case of a mortified bowel, why may not the same rule apply to a *large portion of omentum* in the same state, more especially, as there is reason to believe that the living omentum situated within the parietes of the wound, will soon adhere to them, and render the cavity of the abdomen complete.

Happily for us, those cases do not very often occur. The present one has issued in a most favourable manner. Yet, if it should appear from the experience, but not the speculation of others, that another plan is preferable, I can say with the utmost sincerity, that I should rejoice to adopt any improvement calculated to mitigate the sufferings of afflicted humanity.

Philadelphia, 9th month, 10th.

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### *Case of Premature Labour artificially induced.*

BY Dr. T. C. JAMES.

IN June 1804 I was requested to see, in consultation with the late Dr. Church of this city, a young married lady living in Fourth near South street, then in labour with her first child. Upon visiting her, I found that she had suffered several hours with the severest pains, which had then ceased to produce any progress in the labour.

The pelvis having, upon examination, been found to be preternaturally small; her strength failing; and some threatening symptoms having occurred, it was ultimately agreed, as the head presented, to proceed, as a dernier though painful resource, to open it and deliver by the *crochet*. This was accordingly accomplished, although with some difficulty.



She recovered from the parturient state as rapidly as we could have expected or even wished; and as in the course of examination during the progress of delivery we had satisfactorily ascertained the preternaturally small dimensions of the pelvis, we gave it as our joint opinion, upon being interrogated on the subject by the friends of the lady, that she could not bear a full grown living child at the full period of utero-gestation.

Her husband dying not long after, she did not again become pregnant until after a second marriage, when Dr. Mongez was engaged to attend her during parturition. She accordingly fell in labour in August 1809, and I was again called into consultation on her case. She had been in labour upwards of twenty-four hours, when, from our previous knowledge of her case, from the comparative large size of the head of the child, and from several unpleasant symptoms in our patient, we thought ourselves called upon, although with sentiments of extreme regret, again to make use of the crotchet.

With considerable and long continued exertions on the part of both of us, we ultimately succeeded, after several hours, in delivering her of the child, which most probably had been dead some time, as we were induced to conclude from existing marks, which could not readily have deceived us.

Upon our opinion being requested we again agreed to state, that our patient probably could never bear a living child at the full period of utero-gestation. We therefore advised, that if she should again find herself in that situation, she should adhere to an abstemious regimen, be occasionally bled, and from time to time take a cathartic; and that at the period of seven months from conception, premature labour should be induced by rupturing the membranes. We were justified in this plan by the respectable authorities of Denman, Barlow, and several other experienced accoucheurs.

Dr. Mongez saw the patient from time to time, and informed me in the spring of the present year [1810] that she was again pregnant, and that both herself and her friends were anxious that the plan we had proposed should be carried into effect. Not long after she called upon me, and appeared to be willing that the experiment should be tried.

On the 26th of June, 1810, when, according to the most accu-

rate account we could procure, she was at the end of the seventh month of pregnancy, we examined her *per vaginam*, and during the examination found the os uteri dilating to nearly the size of a half dollar, and that the oviform presentation of the membranes could be distinctly felt.

As she was to be removed from Queen street, where she then dwelt, to her mother's house in Union, near Fourth, previously to rupturing the membranes, we contented ourselves with recommending venesection and an enema for the present. On the 27th she was removed to her mother's, and on the evening of that day I was informed by Dr. Mongez that the membranes had spontaneously ruptured with very little pain.\*

On the 29th in the morning, about 8 o'clock, I was requested by the husband to meet Dr. Mongez at her mother's house, which I accordingly did. We found that labour had commenced, and upon examination, about half past 9 A. M. the os uteri was found to be considerably dilated, so that the presentation could be clearly distinguished, the posterior fontanelle being towards the left acetabulum.

We agreed, as there were no reasons to justify precipitation, to suffer the labour to proceed, and by about half past ten A. M. she was delivered of a living female child, which, as we had previously calculated, appeared to be at the period of seven months from conception.

By the third of July the secretion of milk was fully established. The child took nourishment by the spoon, and attempted to suck, but not with as much success as we could have wished. It notwithstanding lived until the 19th of the month, when it expired, partly, we believe, in consequence of mismanagement in the nursing. The mother happily recovered.

Dr. Denman, who appears to have paid particular attention to this subject, observes, that the first account of any artificial method of bringing on premature labour was given to him by Dr. Kelly. In the year 1750 the propriety of this plan appears

\* This spontaneous rupture, if it may be so termed, must be attributed to the irritation produced by the examination made the day preceding; Dr. Mongez having then, as he informed me, after the partial dilatation of the os uteri, endeavoured to scratch the membranes with the nail of his forefinger. Hence contraction of the uterus must have been in a degree produced, and the rupture of the membranes necessarily facilitated.



to have been sanctioned by a consultation of the most eminent practitioners in London, and the first case in which it was deemed necessary and proper, and which terminated successfully, fell under the care of Dr. Macaulay, much celebrated at that period as an accoucheur.

Since which Dr. Denman, having carefully attended to all the circumstances which had occurred when it had been performed in more than twelve cases, in which he had either performed it or where it had been done by his advice and persuasion, he has not, he says, known one untoward or hazardous accident that could be imputed to it, and in the greater number of those cases the children have been born living, whilst it has been perfectly safe to the person on whom it has been performed.

The respectable physician whom we have just quoted has recommended also bringing on premature labour in those women who readily conceive, proceed regularly in their pregnancy till they approach the full period, when, without any apparent cause, they have been repeatedly seized with rigours, and the child has instantly died, though it may not have been immediately expelled. In two cases of this kind he proposed bringing on premature labour, and succeeded in preserving the children without hazard to the mothers. There must of course, in these cases, as he acknowledges, be always something of doubt, whether the child might not have been preserved without the operation.

The next practitioner of character, whose writings I have met with, who recommends bringing on premature labour, in cases where the diameter of the pelvis is preternaturally small from distortion, is John Barlow, surgeon at Bolton, in Lancashire, England. This gentleman, in a paper that he published in the eighth volume of the *Medical Facts and Observations*, details several cases which proved the propriety of the practice which he appears to have pursued during several years with success. He was in the habit of exciting premature labour early in the seventh month, whenever he was consulted in time by distorted patients. In many of these cases the children were born living, and in some lived for several years.

The great objects of this operation, if such it may be termed, would appear to be to obviate the necessity of the Cæsarian

section, by which the life of the mother is brought into the greatest possible danger on the one hand, and on the other, to prevent the occasion of embryulcia, by which the infant is necessarily sacrificed in a manner the most abhorrent to the feelings of the practitioner.

Authors have stated that the dimensions of the pelvis that require and justify the inducing premature labour are from two to two and a half inches from pubes to sacrum; but as it is often difficult if not impracticable to determine the precise dimensions of the pelvis or of the child's head previously to delivery, and of course the precise relations between them must be unknown, it has therefore been judiciously said by Dr. Denman, that "under circumstances and in situations just preventing the successful use of the forceps, and just compelling us to the fatal measure of lessening the child's head, it may become a duty to propose on a future occasion the bringing on premature labour; at seven months or at any later time, according to our sense of the disproportion existing between the head of the child and the cavity of any particular pelvis."

With respect to the *time* of inducing labour, we may just be permitted to observe, that, as a general rule, the operation should not be performed sooner than the end of the seventh month, and that the eighth month may often be considered as preferable. Dr. Denman relates a case in the third volume of the *Medical and Physical Journal*, where it was performed about midway between the seventh and eighth month, and where the child lived, a wet nurse having been previously provided, which, in fact, should always be done, for reasons which are too obvious to need recital.

Philadelphia, August 6, 1810.



## ORIGINAL REVIEW.

*Cases of Organic Diseases of the Heart, with Dissections, and some Remarks intended to point out the Distinctive Symptoms of these Diseases, read before the Counsellors of the Massachusetts Medical Society.* By John C. Warren, M. D. 8vo. pp. 61. printed at Boston by Thomas B. Wait & Co. 1809.

IN this work Dr. Warren has presented the public with the history of ten cases, where the dissections showed the heart to have undergone considerable derangement in its structure. Some of the cases are related with a minuteness which will be acceptable to every one, who wishes to become acquainted with the symptoms which attend the diseases of this important organ. The dissections, with one exception, appear to have been made by Dr. Warren, and are highly creditable to his zeal, and to the liberality of the people among whom he exercises his profession. The following extract contains the result of Dr. Warren's observations, and will best explain his views and ideas of the organic diseases of the heart, and of the symptoms by which such affections may be distinguished from those disorders with which they have been frequently confounded.

"ENUMERATION of the principal morbid changes, observed in the organization of the heart, in the preceding cases.

Enlargement of the volume of the heart, or aneurism.\*

Increase of the capacity, or aneurism of the right auricle,	} with thickened, or thin, parietes.
..... of the right ventricle,	
..... of the left auricle,	
..... of the left ventricle,	
..... of the aorta, with thickening of its coats.	

Fleshlike† thickening of the mitral valves.

..... of the aortal valves.

..... of the aorta.

Cartilaginous thickening of the internal membrane of the heart, and generally of its valves.

\* Morgagni uses this term, which he borrows from Ambrose Paré, to express dilatation of the cavities of the heart. It seems to be as applicable to the dilatation of the heart, as to that of an artery. I have therefore adopted it in this enumeration.

† The term fleshlike is employed to express that roughness of the valves, which somewhat resembles flesh in its appearance, but which is very different from the thickening of the parietes of the heart.

Ossification of the parietes of the heart.

- ..... mitral valves.
- ..... aortal valves.
- ..... aorta.
- ..... coronary arteries.

*"ENUMERATION of the principal morbid appearances, observed in these cases of disease of the heart, which may be considered secondary.*

**IN THE CAVITY OF THE CRANIUM.**

- Inflammation of the meninges.
- Water between the meninges.
- Water in the ventricles.

**IN THE PLEURA AND ITS CAVITY.**

- Inflammation and thickening of the pleura.
- Collection of water in its cavity.
- Lungs dark coloured.
- ..... generally very firm, and particularly in some parts.
- ..... loaded with black blood.
- ..... crowded into a narrow space.

**IN THE PERICARDIUM AND ITS CAVITY.**

- Inflammation and thickening of its substance.
- Adhesion to the heart and lungs.
- Collection of water in its cavity.

**IN THE CAVITY OF THE ABDOMEN.**

- Collection of water.
- Liver very full of fluid blood.
- ..... having its tunic flaccid and inflamed.
- Mesenteric veins full of blood.

**CELLULAR MEMBRANE** full of water.

**THE BLOOD** every where fluid, except in the cavities of the heart.

**REMARKS.**

"The symptoms, which are most observable, in some or all of the preceding cases, are the following:

"The first notice of disorder is commonly from an irregular and tumultuous movement of the heart, which occurs some time before any perceptible derangement of the other functions. This irregularity slowly increases, and arrives at its height before the strength of the patient is much impaired, at least in the cases which I have noticed; and as the vigour of the patient lessens, the force of the palpitations diminishes. These palpitations are often so strong, as to be perceptible to the eye at a considerable distance. They are seldom most distinct in the place where the pulsation of the heart is usually felt.



Sometimes they are perceived a little below; often in the epigastric region; and not unfrequently beneath, and on the right side, of the sternum.

“After the palpitations have lasted some time, a little difficulty of breathing, accompanied with sighing, is perceived, especially on any great exertion, ascending an eminence, or taking cold, of which there is an uncommon susceptibility. This dyspnœa becomes, as it increases, a most distressing symptom. It is induced by the slightest cause; as by an irregularity in diet, emotions of the mind, and especially movement of the body; so that on ascending stairs quickly, the patient is threatened with immediate suffocation. It occurs at no stated periods, but is never long absent, nor abates much in violence during the course of the disease. It is attended with a sensation of universal distress, which perhaps may arise from the circulation of unoxygenated blood, or the accumulation of carbon in the system; for the countenance becomes livid, and the skin, especially that of the extremities, receives a permanent dark colour. This dyspnœa soon causes distress in lying in a horizontal posture. The patient raises his head in bed, gradually adding one pillow after another, till he can rarely, in some cases never, lie down without danger of suffocation; he inclines his head and breast forward, and supports himself upon an attendant, or a bench placed before him. A few hours before death the muscular power is no longer capable of maintaining him in that posture, and he sinks backward. The dyspnœa is attended with cough, sometimes through the whole of the disease, sometimes only at intervals. The cough varies in frequency. It is always strong, and commonly attended with copious expectoration of thick mucus, which, as the disease advances, becomes brown coloured, and often tinged with blood; a short time before death it frequently consists entirely of black blood.

“The changes in the phenomena of the circulation are very remarkable. The sanguiferous system is increased in capacity; the veins, especially, are swelled with blood; the countenance is high coloured, except in fits of dyspnœa, when it becomes livid; and it is very frequently puffed, or turgid. The brightness of the eyes, dizziness, which is a common, and headache, which is a frequent symptom, and in some cases very dis-

tressing, are probably connected with these changes. The motions of the heart, as has already been stated, are inordinate, irregular, and tumultuous. The pulse presents many peculiarities. In some cases, probably where there is no obstruction in the orifices of the heart, it remains tolerably regular, and is either hard, full, quick, vibrating and variable, or soft, slow, compressible and variable. Most commonly, perhaps always, when the orifices of the heart are obstructed, it is vibrating, very irregular, very intermittent, sometimes contracted and almost imperceptible, very variable, often disagreeing with the pulsations of the heart, and sometimes differing in one of the wrists from the other.

"The functions of the brain suffer much disturbance. Melancholy, and a disposition for reverie, attend the early stages of the complaint; and there is sometimes an uncommon irritability of mind. The dreams become frightful, and are interrupted by sudden starting up in terror. Strange illusions present themselves. The mental faculties are impaired. The termination of the disease is attended with slight delirium; sometimes with phrensy, and with hemiplegia.

"The abdominal viscera are locally, as well as generally, affected. Although the digestive functions are occasionally deranged, the appetite is at some periods remarkably keen. The action of the intestines is sometimes regular, but a state of costiveness is common. The liver is often enlarged, probably from accumulation of blood. This distention is attended with pain, varies much, and, in all the cases I have seen, has subsided before death, leaving the coats of the liver wrinkled, flaccid, and marked with appearances of inflammation, caused by the distention and pressure against the surrounding parts. An effect of the accumulation of blood in the liver, and consequently in the mesenteric veins, is the frequent discharge of blood from the hæmorrhoidal vessels. This occurs both in the early and late stages of the disease, and may become a formidable symptom. Evacuations of blood from the nose are not uncommon.

"Dropsical swellings in various parts of the body succeed the symptoms already enumerated. They commence in the cellular membrane of the feet, and gradually extend up the legs and thighs; thence to the abdominal cavity, to the thorax, some-



times to the pericardium, to the face and superior extremities; and, lastly, to the ventricles and meninges of the brain. These collections of water may be reabsorbed by the aid of medicine; but they always return and attend, in some degree, the patient's death.

"There is no circumstance more remarkable in the course of this complaint, than the alternations of ease and distress. At one time the patient suffers the severest agonies, assumes the most ghastly appearance, and is apparently on the verge of death; in a day or a week after, his pain leaves him, his appetite and cheerfulness return, a degree of vigour is restored, and his friends forget that he has been ill. The paroxysms occasionally recur, and become more frequent, as the disease progresses. Afterwards the intermissions are shorter, and a close succession of paroxysms begins. If the progress of the complaint has been slow, and regular, the patient sinks into a state of torpor, and dies without suffering great distress. If, on the contrary, its progress has been rapid, the dyspnœa becomes excessive; the pain and stricture about the præcordia are insupportable; a furious delirium sometimes succeeds; and the patient expires in terrible agony."

Dr. Warren considers "the incurvation of the body forward" as pathognomonic of organic diseases of the heart. Among the various medical records which we have consulted, we have met but two cases where this symptom is noticed. In both the pericardium was found filled with blood. One case of violent pneumonic disease, which has occurred periodically for some years, has come to our knowledge; during the attacks the person finds no ease but when sitting with his head bent forward and resting on a pillow. We do not wish to call in question the accuracy of Dr. Warren's observation; but are desirous of inviting inquiry, and shall be happy to learn that what he has stated is confirmed by the experience of others.

Two plates executed in a style of great neatness and perspicuity accompany the work. The first represents the inside of the aorta laid open, exhibiting the valves, (which are here but two in number) thickened and ossified. The second plate is the representation of a fleshlike thickening seen on the inner surface of the aorta.

*Medical and Philosophical Intelligence.*

THE following report was read at the last annual meeting of the governors of the London Vaccine Institution.

"The board of managers have the happiness to inform the annual meeting, that the cause of vaccination still continues to flourish under their auspices. This is evinced by the numerous applications for matter, not only in the metropolis, but from all parts of the united kingdom, the colonies and countries abroad.

They have great pleasure in observing the increasing progress of vaccination; their returns being greater than on any former year. It appears that since the last annual report, there have been inoculated by Dr. Walker 2,087; from the beginning 6,105. By the appointed inoculators in the metropolis last year 1,105; from the beginning 2,163. By the appointed inoculators in the country last year, 54,665; from the beginning 156,573.

Dr. Walker, since the last report, has supplied to 3,973 applicants, 19,865 charges of matter. From the beginning, to 12,361 applicants, 61,088 charges.

In having been enabled to effect the foregoing incalculable services to society at large, the board of managers have to acknowledge a liberal support from a generous public; yet they owe to that public, and to the cause of vaccination, the statement of the fact, that the support of this most extensively useful establishment requires still further contributions, to enable the managers to completely effect the vast, patriotic and philanthropic plan.

It is to be lamented, that the fatal malady which has, during the last thousand years, committed ravages the most dreadful in every quarter of the world, is not extinct in the metropolis. The board of managers, however, request the attention of every member of the institution to this most important and gratifying circumstance; namely, that while 1163 have died of the small-pox, during the last year, as appears from the bills of mortality, not a single instance of the kind is represented as having occurred from vaccination."



*Bohan Upas, or Poison Tree of Java.*

AT a late meeting of the Royal Society, the translation of a paper by M. Delille was read, describing the real nature and properties of the celebrated Bohan Upas, or poison tree of Java. The author, a French physician, and a member of the National Institute of Egypt, transmitted this paper from the East Indies to the Royal Society, by an English lady. The botanical account of the plant in question, he received from one of the French naturalists who accompanied captain Baudin, and who resided some time in Java, where he visited the interior of the country, and with much difficulty prevailed on the natives to show him the different poison plants, which they carefully conceal, for the purpose of using them in war. Hence the many fabulous accounts that have been circulated respecting the fatal influence of the Upas; which in the language of the Javanese signifies vegetable poison, and is applied only to the juice of the Bohan Tree, and another plant with a twisted stem. The former is a large tree, which the writer considers as a new genus; the latter, yielding an equally powerful poison, is of the woodbine family. The Upas, or juice, is extracted by an incision made in the bark with a knife, and being carefully collected, is preserved by the natives to be employed in their wars. As to its diffusing noxious effluvia in the atmosphere, and destroying vegetation to a considerable distance around it, the absurdity of these stories is sufficiently exposed by the fact, that the climbing species requires the support of other plants to attain its usual growth. Dr. Delille made several experiments with the Upas on dogs and cats. An incision was made in the thigh of a dog, into which were dropped eight grains of the juice. The dog soon began to vomit, and continued vomiting at intervals till he became convulsed, and died in twenty minutes. Six grains were put into the thigh of another, which was seized with the same symptoms, and died in fifteen minutes. A cat was treated in like manner, but the effects were more powerful and speedy; she expired in a few minutes. All these animals died howling and in great agony. The author also made several experiments on the effects of this poison when applied internally. A grain and a half being introduced into the stomach of a dog, produced

only a slight purging. To another were given four grains, which in about four hours produced the same effect, together with vomiting, and the dog died in the course of half a day. On examining the bodies of these animals after death, no very extraordinary appearances were discovered; the ventricles of the heart were full of blood, and some slight traces of inflammation appeared in the stomach; but the derangement was not so great as might have been expected from such a violent and sudden death. From this circumstance the author concluded, that the absorbents had transmitted the poison to the nerves of the stomach, and that this peculiar species of vegetable poison acts exclusively on the nerves.

*Pearson's Experiments on Expectorated Matter.*

IN June, 1809, a paper was read before the Royal Society of London, on the matters raised from the lungs by human beings. These Dr. Pearson arranges under seven heads: 1. The jelly-like, semi-transparent kind, of a blueish hue, excreted in the healthy state. 2. The thin mucilage-like transparent matter, so copiously expectorated in bronchial catarrhs. 3. The thick, opaque, straw-coloured or white and very tenacious matter, coughed up in a great variety of bronchial and pulmonary affections; especially in that of tubercles. 4. Puriform matter secreted without any division of continuity or breach of surface of the bronchial membrane, very commonly occurring in pulmonary consumptions. 5. The matter which consists of opaque viscid masses, together with transparent fluid, or the second sort above stated, with nodules of the third or fourth kind. 6. Pus from the vomicae of tubercles; and 7. Pus from vomicae by simple inflammation of the lungs, and without tubercles. From a variety of experiments made on the five former of these substances, by caloric, alcohol, water, vinegar, and other agents, Dr. Pearson concludes that the several sorts of expectorated matter do not differ in the ingredients of their composition, but merely in the proportion of them to one another. He shows that they chiefly consist of *coagulable* or *albuminous* animal substances, instead of mucus, as heretofore believed; of which about one twelfth or one tenth may be called an animal oxyd; and a considerable proportion is eva-



porable water, impregnated with several saline and earthly bodies. Besides muriate of soda, he found potash in these expectorated fluids: to wit, from half to three fourths of a part per thousand of potash, while the common salt varied from one and a half to two and a half parts in a thousand; and the neutralized potash he also found in the blood, dropsy fluid, pus of abscesses, pus secreted without breach of surface, the serum effused from blisters, urine, and the slimy secretions from the nostrils in catarrhs. So that potash may now be ranked among the constituent parts of animal matter.

*University of Pennsylvania.*

THE Trustees of the University of Pennsylvania, with a view to a more complete system of *Medical Education* in that seminary, have lately established a professorship of NATURAL PHILOSOPHY for the medical department, and divided the professorship of ANATOMY and MIDWIFERY into two separate professorships.

Dr. Caspar Wistar is elected the professor of Anatomy; Dr. Thomas C. James the professor of Midwifery; and Robert Hare, jun. esq. the professor of Natural Philosophy.

*College of Physicians of Philadelphia.*

AT a stated meeting of the College of Physicians, held on the third of July 1810, the following gentlemen were chosen officers for the ensuing year.

Dr. Adam Kuhn, president.

Dr. Samuel Duffield, vice-president.

Dr. Thomas C. James, treasurer.

Dr. Thomas T. Hewson, secretary.

Dr. Thomas Parke,

Dr. Samuel P. Griffiths,

Dr. Caspar Wistar,

Dr. William Currie,

} Censors.

## Foreign Publications.

### NEW WORKS AND RECENT EDITIONS.

#### *Practice of Medicine.*

PRACTICAL Observations on Disorders of the Stomach, with Remarks on the use of the bile in promoting Digestion. By George Rees, M. D. member of the Royal College of Physicians, senior physician to the London Dispensary, &c. &c. Price 6s. in boards.

An Essay on the Nature of Scrofula; with evidence of its origin from disorder of the digestive organs; illustrated by a number of cases successfully treated, and interspersed with observations on the general treatment of children. By Richard Carmichael, surgeon. 8vo. 5s. boards.

A Practical Essay on Cancer; being the substance of Observations to which the Annual Prize for 1808, was adjudged by the Royal College of Surgeons, London. By Christopher Turner Johnson, surgeon of Exeter, member of the Royal College of Surgeons of London, and of the Royal Medical Society of Edinburgh. 8vo. 5s. 6d.

Observations on the Hydrargyria; or that Vesicular Disease arising from the exhibition of Mercury. By George Alley, M. D. M. R. I. A. Fellow of the Royal College of Physicians of Edinburgh. Embellished with three engravings. 4to. 14s. boards.

The Modern Practice of Physic: exhibiting the characters, causes, symptoms, prognostic, morbid appearances, and improved method of treating Diseases. By Robert Thomas, M. D. of Salisbury. A third edition, considerably enlarged, in one neat and very close printed volume, 8vo. 14s. This work has been again carefully revised; and a large portion of new and important matter has been added.

A Practical Treatise on the Venereal Disease, describing the nature, symptoms, and cure of the Diseases arising and connected with it; third edition, illustrated with cases. By T. M. Caton, surgeon. 4s. 6d.

A Treatise on Local Inflammation, more particularly applied to Diseases of the Eye. By T. B. Stern, M. D. Oculist.

Advice to Mothers, of the greatest importance in the Treatment of Children during Infancy; from the German of Hufeland. A new edition. 2s. 6d. sewed.

An Essay on the Nature of Fever; being an attempt to ascertain the Principles of its Treatment. By A. Philips Wilson,



M. D. F. R. S. Ed. Fellow of the Royal College of Physicians of Edinburgh, &c. 8vo. 5s.

Letters on the Cause and Treatment of the Gout, with Remarks on other medical subjects. By Robert Hamilton, M. D. of Lynn. Fellow of the London and Edinburgh Colleges. 5s. boards.

A Practical Treatise on Tinea Capitis Contagiosa, and its cure. By William Cooke, surgeon.

A familiar Essay, explanatory of the Theory and Practice of Prevention of Venereal Contagion. By a member of the Royal College of Surgeons, London, royal 8vo. 10s. 6d.

A Scientific and Popular View of the Fever of Walcheren, and its consequences, as they appeared in the British troops returned from that country. By J. R. Davis, M. D. 8vo. 7s.

An Inquiry into the Nature, Causes, and Cure of Hydrothorax: illustrated by interesting cases, and many examples of the success of the mode of treatment recommended. By C. M<sup>c</sup>Clean, M. D. 8vo. 12s.

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THE  
ECLECTIC REPERTORY  
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Vol. I.

JANUARY, 1811.

No. II.

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SELECTED PAPERS.

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*The Bakerian Lecture for 1809.*

On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.

BY HUMPHRY DAVY, Esq. Sec. R. S. F. R. S. E. M. R. I. A.

From the Philosophical Magazine.

*I. Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries on the application of Electricity to Chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views, and applications, not unconnected with the objects of the Bakerian Lecture: and though many of them are far from having attained that precision, and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some impor-

Vol. I.

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tant and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

## II. *Some new Experiments on the Metals from the fixed Alkalies.*

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecomposed, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phenomena.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of inquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phenomena, different from those I adopted.

MM. Gay Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda with hydrogen; a similar opinion seems to be entertained by M. Ritter. M. Curaudau\* affects to consider them as combinations of charcoal, or of charcoal and hydrogen, with the alkalies; and an Inquirer† in our own country regards them as composed of *oxygen* and *hydrogen*.

I shall examine such of those notions only as have been connected with experiments, and I shall not occupy the time of the society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which MM. Gay Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the *Moniteur*;

\* *Journal de Physique*, June 1808.

† Nicholson's Journal, August 1809, p. 258.

and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the *Mem. d'Arcueil*, a copy of which M. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail, it seems that they still retain their opinion; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the society, I shall venture to state fully their method of operation, and of reasoning.

They say that they heated potassium\* in ammonia, and they found that a considerable quantity of ammonia was absorbed, and hydrogen produced; and that the potassium became converted into an olive-coloured fusible substance; by heating this substance strongly, they obtained three-fifths of the ammonia again, two-fifths as ammonia, one-fifth as hydrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two-fifths, and found in the vessel in which the operation was carried on, nothing but potash.—Again, it is stated, that by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm that they can procure from a given quantity of ammonia, more than its volume of hydrogen.

Whence, they ask, can the hydrogen proceed? Shall it be admitted that it is from the ammonia? But this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. Berthollet, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogen gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogen, and that alkali.

It is obvious, that even supposing the statement of these gen-

\* *Mem. d'Arcueil*, tom. ii. p. 309.



lemen correct, their conclusions may easily be controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxygen which this would give to the potassium, and of the hydrogen which it might furnish, to reproduce the ammonia, they take no notice.

I have shown, by numerous experiments, many of which have been repeated before members of this society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion is less ammonia regenerated; and I have seldom obtained as much as  $\frac{1}{10}$  of the quantity absorbed. And I have never procured hydrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions, [Phil. Mag. vol. xxxiv. p. 344.] and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from three to four grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in these cases there was a considerable deficiency of hydrogen, as well

as nitrogen, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases I obtained more than four-fifths of the potassium which had been employed; and very nearly the whole of the nitrogen, existing in the ammonia that had been acted upon.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at  $30\cdot2^{\text{in}}$ , thermometer at  $54^{\circ}$  Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia,  $7\cdot5$  were absorbed, and  $3\cdot2$  of hydrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors, was exhausted, and filled with hydrogen. They contained together  $\frac{8}{10}$  of a cubical inch. The heat was very slowly applied by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and  $\frac{1}{3}$  of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches,  $\frac{1}{3}$  of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with  $7\cdot5$  of oxygen, and acted upon by the electrical spark, left a residuum of  $7\cdot5$ . The quantity of potassium formed, was such as to generate by its action upon water three cubical inches and  $\frac{3}{10}$  of hydrogen gas.

Now if this experiment be calculated upon, it will be found, that  $7\cdot5 - 2 =$  to  $7\cdot3$  of ammonia, by its electrical decomposition, would afford about 13·1 of permanent gas, containing 3·4 of nitrogen, and 9·7 of hydrogen. But the  $3\cdot2$  cubical inches of hydrogen, evolved in the first part of the process, added to the  $5\cdot8$  evolved in the second part of the process,  $= 9$ ; and the nitrogen in the 8·8 cubical inches of gas (or the  $9 - 2$  of ammonia) will be about 3, and if we estimate  $\cdot34$  of hydrogen, and  $\cdot16$  of nitrogen, in the  $\cdot5$ , remaining in the retort, there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the  $\frac{8}{10}$  of hydrogen preexisting in the tube and adaptors, the loss of hydrogen will be found proportionally rather greater than that of nitrogen.

In another experiment in which three grains of potassium were employed in the same manner,  $6\cdot78$  cubical inches of am-



monia were found to be absorbed, and 2.48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors, the same quantity of residual air as in the process last described.

The 8 cubical inches of gas contained scarcely  $\frac{2}{3}$  of a cubical inch of ammonia; and the unabsorbable part detonated with oxygen, in the proportion of 11 to 6, gave a residuum of 7.5.—The barometer was at 30.2<sup>in</sup>, thermometer at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6.78—4 of ammonia=6.38, and this quantity of gas decomposed by electricity, would afford 11.4 of permanent gas, consisting of 2.9 nitrogen, and 8.5 hydrogen; but there are produced in this experiment, of hydrogen, 2.48 in the first operation, and 4.28 in the second, and considering the nitrogen in the permanent gas as 3.32, .8 must be subtracted for the common air; which would give 2.52 for the nitrogen generated; and to these must be added, the quantity of hydrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2.9 cubical inches of hydrogen.

In all experiments of this kind, a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort containing oxygen gas; there was a diminution of the gas; and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from three

grains of potassium were made to act upon ammonia in contact with platina, and afterwards distilled in contact with platina. The results were always from four to six grains of this powder, but I have advanced no further in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxygen.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture, and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hydrogen. When copper tubes are employed, the hydrogen bears a smaller proportion to the nitrogen, and more potassium is revived.

In these experiments, in which platina has been used, there is little or no loss of potassium or nitrogen; but a loss smaller or greater of hydrogen.

It will be asked, on what do these circumstances depend? Does the affinity of certain metals for potassium prevent it from gaining oxygen from ammonia, and do platina and copper combine with a small quantity of hydrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not a *metallic* body that is decomposed but the volatile alkali, and that the *hydrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygen and hydrogen.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments



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just detailed, a tray, and the same tube of platina being employed.

$3\frac{3}{10}$  grains of sodium I found absorbed 9.1 of ammonia, and produced about 4.5 of hydrogen, and the fusible substance, which was very similar to that from potassium, distilled, did not give off  $\frac{1}{20}$  of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced, equalled twelve cubical inches, and, by detonation with oxygen, proved to consist of nearly two of hydrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention, the mere visible phenomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal, which is decomposed in this process.

As sodium does not act so violently upon oxygen, as potassium; and as soda does not absorb water from the atmosphere, with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green, it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form, evidently, by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogen.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. Curaudau, that the metals of the alkalies are composed of the *alkalies* merely united to *charcoal*; the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which MM. Gay Lussac and Thenard have been misled by the moisture which interfered with their results.

M. Curaudau states, that when sodium is oxidated, carbonic

acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in eight cubical inches of oxygen: nearly two cubical inches of oxygen were absorbed, and soda in a state of extreme dryness, so that it could not be liquefied by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phenomena which I have described in the Bakerian lecture for 1807. Nearly six cubical inches of hydrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination; and it appears from M. Curaudau's method of decomposing the alkalies, that his metals must have been carburets, not of potash and soda, but of potassium and sodium.

M. Ritter's argument, in favour of potassium and sodium being compounds of hydrogen, is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxygen than potassium, and on the hypothesis of hydrogenation, must contain much more hydrogen; yet though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly,\* yet sodium is heavier than potassium in the proportion of 9 to 7 at least.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxygen than sodium; and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

M. Ritter has stated, that of all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of elec-

\* *Haasenfratz, Annal. de Chim. tome xxvii. p. 11.*



tricity is completed in water, by means of two surfaces of tellurium, oxygen is given off at the positive surface, no hydrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogen than that alkali.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find that instead of proving that potassium is a compound of potash and hydrogen, they confirm the idea of its being as yet like other metals undecomposed.

When tellurium is made the positive surface in water, oxygen is given off; when it is made the negative surface, the Voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opaque and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hydrogen in water; which, in being diffused, is acted upon by the oxygen of the common air, dissolved in the water, and gradually loses a part of its hydrogen, and becomes a solid hydruret of tellurium. The compound of hydrogen and tellurium produced at the negative pole when uncombined is gaseous at common temperatures, and when muriatic acid or sulphuric acid are present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large Voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found annexed to this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which when touched by water, did not inflame nor effervesce,

but rendered the water of a beautiful purple colour, and when thrown into water entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hydrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium, and that the *telluretted* hydrogen (if the name may be used) had formed with the oxidated potassium, *i. e.* the potash, a peculiar compound, soluble in water; and this I found to be the case; for on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hydrogen; metallic tellurium was formed where it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was further confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogen was evolved by the action of the alloy upon water; but the compound of telluretted hydrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other, induced me to conceive that the *decomposition of potash* might be easily effected, by acting on the oxide of tellurium and potash at the same time, by heated charcoal; and I soon proved that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.



In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hatchett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aëriform* compound of tellurium and hydrogen; I shall mention merely some of its most remarkable qualities, and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hydrogen is more analogous to sulphuretted hydrogen, than to any other body. The smell of the two substances is almost precisely the same.\* Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through

\* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children, Esq. of Tunbridge, in which Mr. Children, Mr. Pepys, and Mr. Warburton cooperated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of Voltaic electricity, or by potassium; and some researches made upon the habitudes of different metallic sulphurets, at the Voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hydrogen in the Voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxygenation could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and further researches, made in the laboratory of the Royal Institution, proved that the substance in question was a new and singular combination.

it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic, but which is soon converted into muriate of tellurium.\*

As arsenic has an affinity for hydrogen, it occurred to me as probable, that it would present some phenomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark-coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogen by the action of water with inflammation, and deposited a brown powder.

When potassium and arsenic† were heated together in hydrogen gas, they combined with such violence as to produce the phenomena of inflammation, and an alloy was produced of the same kind as that formed by means of the Voltaic battery.

\* From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogen, enters into combination with it. An accident prevented me from ascertaining whether the compound so formed, is exactly the same as that described in the text.

† In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, Fourcroy *Connais Chem.* tom. viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that though potash is decomposed in this operation, yet that the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogen. The gases not absorbable by water given off in this operation, are peculiar. Their smell is intensely fetid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogen: whether they are mixtures of various gases, or a single compound, I am not at present able to decide.



As tellurium and arsenic both combine with hydrogen, it appeared to me probable, that by the action of alloys of potassium, with tellurium and arsenic, upon ammonia, some new phenomena would be obtained, and probably, still further proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendor, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four-sixths nitrogen, instead of being pure hydrogen, as in the case of the action of potassium alone.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas which was principally nitrogen; so that if it be said that the metal, and not the volatile alkali, is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hydrogen; which are contradictory assumptions.

None of the chemists who have speculated upon the *imaginary hydrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in which agents, which they did not suspect were concerned. No person, I believe, has attempted to show that when potassium or sodium is burnt in oxygen gas, water is formed, or that water is generated when potassium decomposes any of the acids;\* and no one has been able to form potassium, by combining hydrogen with potash. I stated

\* When in October 1807, I obtained a dark-coloured combustible substance from boracic acid, at the negative pole in the Voltaic circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution March 12. Soon after I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. MM. Gay Lussac and Thenard have likewise operated upon boracic acid, by potassium, and they conclude that they have decomposed it; but this does not

in the Bakerian lecture for 1807, that when potassium and sodium were burnt in oxygen gas, the pure alkalies were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts of oxygen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed; yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find that this is the case; for the results differ very little in some experiments which I have made upon considerable portions of potassium and sodium, procured by chemical decomposition.

When potassium is burnt in trays of platina, in oxygen gas that has been dried by ignited potash, the absorption of oxygen is about  $\frac{1}{8}$  of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain.\* The alkalies so formed, are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'Arcet has shown by some very well conducted inquiries, that potash and soda,† in their common state, contain a considerable proportion of water; and M. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13.89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been ignited, contains in the 100 parts 66.66 potash, and 33.34 muriatic acid, a determination which differs very little from that of Bucholz.

To determine the relation of the dryness of the potash,

follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash: the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hyduret of boracic acid.

\* The quantities of gas given out by the operation of water, are in a similar ratio. See page 43 of the last Bakerian lecture [Phil. Mag. vol. xxxii. p. 432.] and page 26 of this paper [p. 136 and 137 preceding.]

† *Annales de Chimie*, Nov. 1803, page 175.



formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances, I obtained from ten parts of potassium 17.5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner: five grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light;\* and the whole mass appeared in igneous fusion; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about five of hydrogen were produced. The increase of weight of the tray was about 4.5 grains; and it did not lose any weight by being ignited.

The second experiment was conducted with still more attention to minuteness. Eight grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phenomena as in the last experiment, and the increase of weight of the tray was  $6\frac{1}{2}$  grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the  $\frac{1}{30}$  of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed

\* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal  $\frac{1}{3}$  of a grain.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1.4 grains of oxygen, to form 9.4 grains of potash, and  $6.6 - 1.4 = 5.2$ , the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35.6 of acid, and 64.4 of potash; but 35.6 of muriatic acid, according to M. Berthollet's estimation, would demand 71.1 of alkali, in the state of dryness in which it exists in muriate of potash, and  $71.1 - 64.4 = 6.7$ —so that the potash taken as a standard by M. Berthollet, contains at least 9 per cent. more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali.\*

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds*, than any of the common *metallic substances*; and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides: this part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

\* Consequently M. Berthollet's fused potash must contain nearly 23 per cent. of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium as the dry standard.



### III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

One of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia, was, that nitrogen might possibly consist of oxygen and hydrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same paths of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists, on the origin of nitrogen, produced during the passage of water through red-hot tubes, or the speculations of Girtanner, founded on these and other erroneous data: the early discovery of Priestly on the passage of gases through red-hot tubes of earthen ware, the accurate researches of Berthollet, and the experiments of Bouillon La Grange, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity.\* To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxygen and hydrogen with great rapidity, and in which these gases could be detonated, without the exposure of the water to the atmos-

\* See Dr. Pearson's elaborate experiments, on the decomposition of water by electrical explosions. *Nicholson's Journal*, 4to, vol. i. page 301.

phere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity that about the eighth of a cubical inch of the mixed gases, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about a half a cubical inch; it had been carefully purged of air by the air pump, and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygen and hydrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about  $\frac{1}{10}$  of the volume of the gases; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than  $\frac{1}{4}$  of the volume of the water, i. e.  $\frac{1}{8}$  of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygen; six measures mixed with three measures of oxygen diminished to five; so that it consisted of 2.6 of hydrogen, and 3.4 of a gas having the characters of nitrogen.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible that at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled  $\frac{3.4}{100}$  of a cubical inch. It was carefully examined: six measures of it, detonated with three measures of oxygen, diminished to rather less than one measure; a result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydro-



gen. That the hydrogen is in excess, may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygen, hydrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxygen and hydrogen, and is never produced from those two gases alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to show that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated.\* The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hydrogen gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogen dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygen gas, and the other in an apparatus in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about  $\frac{1}{12}$  of a cubical inch each, and filaments of asbestos were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gases were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases for from two to three days. In no instance, in which slowly distilled

\* Nicholson's Journal, August 1809, p. 258.

water was employed, and in which the receiver was filled with pure oxygen from oxymuriate of potash, was any acid or alkali exhibited in the cones; even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though, if the asbestos was touched with unwashed hands, or the smallest particle of neutro-saline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestos with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygen and hydrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely that muriatic acid in its arid state may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygen gas. I thought it possible, if water could be combined with *more oxygen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese, made redhot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manga-



nese, no acid was however generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearlsh, that had been ignited by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hydrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterwards distilled from it: figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. Berthollet, that nitrogen adheres very strongly to charcoal.\* The circumstances that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. Priestly upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing there was no notable increase of the volume. At the end of the experiment, about  $\frac{1}{30}$  of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times, in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about  $\frac{1}{20}$  of the volume of the water. It proved to contain about  $\frac{1}{10}$  oxygen,  $\frac{3}{10}$  hydrogen, and  $\frac{6}{10}$  nitrogen.

\* *Mem. d'Arcueil*, tom. ii. page 485.



Mr. Kirwan observed the fact, that when nitrous gas and sulphuretted hydrogen are kept in contact for some time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited, which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results; and I found, that the diminution of the volume of the gases, when they were mixed in equal proportions, was to rather less than  $\frac{1}{4}$ , which seemed to be nitrous oxide.

In reasoning upon this phenomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of this lecture, contains a volume of hydrogen equal to its own. But one of hydrogen demands half its volume of oxygen to convert it into water, and nitrous gas consists of about half a part in volume of oxygen; so that, supposing the whole of the hydrogen employed in absorbing oxygen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogen over mercury, the barometer being at 29.5<sup>in.</sup>, thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gases quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at 30.45<sup>in.</sup>, and thermometer 52° Fahrenheit; when it equalled 2.3. The gas proved to be about  $\frac{3}{4}$  nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds; nitrogen and hydrogen; oxygen and sul-

phur combining to form one; and a part of the nitrogen and oxygen, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompose nitrogen, by agents which I conceived might act at the same time on oxygen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed: but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of Voltaic electricity.

I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant; as soon as the contact with the potassium was made there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, of from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxygen gas.

In all trials of this kind, hydrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference that nitrogen is decomposed in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water and consequently hydrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in



which the greatest precautions were taken; the quantity did not equal  $\frac{1}{11}$  of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen which disappeared in any experiment, was  $\frac{1}{11}$  of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogen, nearly equal to  $\frac{1}{2}$  of the nitrogen, was produced. It cannot be said, that the nitrogen is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogen, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hydrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogen are readily decomposed by oxymuriatic acid gas, a mixture of nitrogen and oxymuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogen; but the greatest part came over densely clouded; and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenor of these inquiries cannot be considered as strengthening, in any considerable degree, the suspicion which I formed of the decomposition of nitrogen, by the distillation of the olive-coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phenomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming that it has been converted into new matter. Though the iron tubes which I used were carefully cleaned; yet still it was not unlikely that a small quantity of

oxide might adhere to the welded parts; the oxygen of which, in the beginning of the process of distillation, might form water with hydrogen, given off from the fusible substance; which, being condensed in the upper part of the tube, would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hydrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly one cubical inch of ammonia, four of nitrogen, and eight of hydrogen. The portion of gas given off in the second operation equalled 3.6 cubical inches; which consisted of 2.5 hydrogen, and 1.1 nitrogen. The potassium produced in the operation was sufficient to generate 3.1 cubical inches of hydrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of M. Hassenfratz,\* that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which, I conceive, could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2.3 cubical inches of hydrogen were found to be generated.

In the experiments detailed in page 53 of the last volume of the Transactions,† a loss of nitrogen, and a production of hydrogen, was perceived in a case in which the residuum from a

\* *Journal des Mines*, Avril 1808, p. 275

† *Phil. Mag.* vol. xxxiii. page 8.



portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible that ammonia might have been regenerated by hydrogen from the *naphtha*, and absorbed by that fluid; and a part of the hydrogen might likewise proceed from the decomposition of the *naphtha*: and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

Even the considerable excess of hydrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451,\* it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me, against the mode of explaining the phenomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygen was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phenomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygen in this substance; at the same time it led to the suspicion, that of the two gases separated by electricity, one, or perhaps both, might contain metallic matter united to oxygen: and the results of the distillation of the fusible substance, from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

\* Phil. Mag. vol. xxxiv. page 339

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an apparatus (of which a figure is attached to this paper) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour, which if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gases,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogen and nitrogen, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive-coloured matter formed on the metal; but though in this case nearly eight cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to  $-15^{\circ}$ , there was a very feeble indication given of the addition of hydrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hydrogen was to the nitrogen in the average proportions of from 73.74 to 27.26; the proper corrections being made, and the precautions before referred to being taken.\*

Assuming the common estimations of the specific gravity of

\* Philosophical Transactions 1809, page 459. M. Bertholett, jun. in the second volume of the *Memoirs of Arcueil*, has given a paper on the decom-



ammonia, of hydrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807 would be supported by these new experiments; but as the moisture and oxygen visibly separated cannot be conceived to be as much as  $\frac{1}{14}$  or  $\frac{1}{12}$  of the weight of the ammonia, I resolved to investigate, more precisely than I had reason to think had been hitherto done, the specific gravities of the gases concerned in their dry state; and the very delicate balance belonging to the Royal Institution placed the means of doing this in my power.

Nitrogen, hydrogen, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at 30.5<sup>th</sup> barometer, 51° Fahrenheit's thermometer.

For nitrogen, the 100 cubical inches.....29.8 grains.

For hydrogen, ditto..... 2.27

For ammonia.....18.4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gases evolved, there is a loss of  $\frac{1}{18}$ ;<sup>\*</sup> and if the smallest proportion be taken, the loss will be nearly  $\frac{1}{12}$ .

position of ammonia, and he enters into an examination of my idea of the oxygen separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent., and at the same time he confutes some experiments which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia. His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxygen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to overrate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible that the volume may be exactly doubled, and that the nitrogen may be to the hydrogen as one to three; but neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

\* 100 of ammonia at the rate of 185, will give 135.9 of hydrogen, weighing 3.1 grains, and 48.1 of nitrogen, weighing 14.33 grains; but 184—17.4=1, and at the rate of 180, 133 of hydrogen weighing 3.01 and 47 of nitrogen weighing 14, and 184—17=1.4.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is *possible*, that the minute quantity of oxygen which appears to be separated is not accidental, but a result of the decomposition; and if hydrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogen and hydrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and



lead; but in all these cases the effect was less distinct than when pure mercury was used.

By exposing the mercury to a cold of  $-20^{\circ}$  Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and, when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from  $\frac{1}{16}$  to  $\frac{6}{10}$  of hydrogen produced.

As it does not seem possible to obtain an amalgam in a uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide) must contain about 48 per cent. of oxygen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases.\*

\* Even in common air, the amalgam evolves hydrogen and ammonia, nearly in these proportions, and in one experiment which I lately tried, there seemed to be no absorption of oxygen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rap-

If hydrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxygen, and 34 of basis; but if the opinion be adopted, that hydrogen and nitrogen are both oxides of the same metal, then the quantity of oxygen in nitrogen must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis, of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hydrogen, and metallic, by combining with a greater proportion.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at  $\frac{1}{12000}$ ; but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56 [page 160 preceding] be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about  $\frac{1}{1600}$  of new matter, on the antiphlogistic view, and about  $\frac{1}{900}$  on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully, these ideas, and I shall conclude this section by stating, that though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet

it idly absorb oxygen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygen had been absorbed; but this difference might have arisen, partly at least, from the mixture of hydrogen. Whether in any case the amalgam absorbs oxygen gas, is a question for further investigation.



they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

#### IV. *On the Metals of Earths.*

I have tried a number of experiments with the hopes of gaining the same distinct evidences of the decomposition of the common earths, as those afforded by the electro-chemical processes on the alkalies, and the alkaline earths.

I find that when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine or glucine, slightly moistened and placed in hydrogen gas; the iron becomes brittle and whiter, and affords by solution in acids, an earth of the same kind as that which has been employed in the experiment.

I have passed potassium in vapor through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali, formed upon the potassium. The potassium\* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence;

\* The results of this experiment are opposed to the idea that potassium is a compound of hydrogen and potash or its basis; for if so, it might be expected that the hydrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogen as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapor of the water, which must have been constantly supplied.

but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour, so that there is great reason to believe, that the silix had been either entirely or partially deoxygenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silix of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silix. The part which did not inflame, was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

Potassium in acting upon alumine and glucine, produced more hydrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable that even after ignition, water adheres to these earths.

The results of the action of the potassium were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks,\* and leaving behind alkali and earth, and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them; which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe

\* The pyrophorus from alum, which I have supposed in the last Bakerian lecture to be a compound of potassium, sulphur, and charcoal, probably contains this substance likewise.



to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury, whilst the tube was hot; but the alkaline metal gave the characters to the amalgam, and though in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded, by solution in acids, evaporation, and treatment with reagents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Though I could not procure decided evidences of the production of an amalgam, from the metals of the common earths, yet I succeeded perfectly by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder, and which when acted upon by weak muriatic acid, gave off hydrogen gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygen which they absorb; and by the solution of the alloys containing the metals

of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

On an hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter, and that oxygen and inflammable bodies are in relations of attraction which correspond to the function of being negative and positive respectively, it would follow that the attractions of acids for salifiable bases would be inversely as the quantity of oxygen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808 concluded that barytes must contain least oxygen of all the earths, and that the order as to the quantity of inflammable matter must be strontites, potash, soda, lime, and so on; and that silix must contain the largest quantity of oxygen of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90.5\* of metal per cent. strontites 86,† lime 73.5,\* magnesia 66.‡

The same proportion would follow from an application of Mr. Dalton's ingenious supposition,§ that the proportion of

\* Mr. James Thomson, *Nicholson's Journal*, 1809, p. 175, and Berthier

† Mr. Clayfield. *Thomson's Chemistry*, vol. ii. p. 626, 629.

‡ *Murray's Chemistry*, vol. iii. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis being inversely as the quantity of oxygen contained by the basis, though gained from the comparison of the electrical relations of the earths, with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which, in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. Gay Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygen, might (as far as it is correct) be inferred from Mr. Dalton's law, though this ingenious chemist states that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxygen, the same in all protoxides, and there is a proportion of acid, the same in all neutral salts; and new proportions of oxygen and of acid are



oxygen is the same in all protoxides, and that the quantity of acid is the same in all neutral salts, *i. e.* that every neutral salt is composed of one particle of metal, one of oxygen, and one of acid.

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition\* of phosphate of alumine, alumine would appear to contain about 56 per cent. of metallic matter.

M. Berzelius,† in a letter which I received from him a few months ago, states, that in making an analysis of cast iron, he found that it contained the metal of silex, and that this metal in being oxidated took up nearly half its weight of oxygen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxygen, which agrees very nearly with the quantity of hydrogen and ammonia produced from the amalgam.

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of

always multiples of these proportions. So that if a protoxide in becoming a deutoxide takes up more acid, it will be at least double the quantity, and in these cases the oxygen will be strictly as the acid. Mr. Dalton's law even provides for cases to which M. Gay Lussac's will not apply, a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxysulphat of iron perfectly formed, (as some experiments which I have lately made seem to show,) there is probably only a single proportion of acid; and in the super-tartrate of potash there is only a single quantity of oxygen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

\* Thomson's Chemistry, vol. ii. p. 581.

† In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

‡ I take the proportions of the volumes from the very curious paper of M. Gay Lussac, on the combinations of gaseous bodies, *Mém. d'Arcueil*, tom. ii. page 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by MM. Gay Lussac and Thenard.

combining with phlogiston, and though Lavoisier insisted upon this analogy with his usual sagacity, yet still the alkalies, earths, and oxides have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls;\* and the alkalies and alkaline earths are both distinguished by their solubility in water; but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalies and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and M. Guyton) precipitate the triple prussiate of potash; the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutro-saline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalies and earths, with oils, to form soaps; and of the earthy soaps, some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxygen, approach very near in their general characters to zircon, silix, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals!

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which,

\* *Elaproph, Annales de Chimie*, tome x. p. 277.



as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold-short iron; whilst, from another spot, it will make hot-short; but, by a combination of the two in due proportion, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, M. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

#### *V. Some Considerations of Theory illustrated by new Facts.*

Hydrogen is the body which combines with the largest proportion of oxygen, and yet it forms with it a neutral compound. This, on the hypothesis of electrical energy, would show that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxygen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phenomena presented by sulphuretted hydrogen are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shown, and as is further proved by the researches of MM. Gay Lussac and Thenard, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decomposed, unless a new combination is formed: thus it is not changed by charcoal ignited

in the gas by Voltaic electricity; but it is decomposed by all the metals; and in these cases hydrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygen being found in the new compound. This, at first view, might be supposed in favour of the idea that hydrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitro-muriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxygenation.

That nitrogen is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches that hydrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus,\* and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the suspi-

\* The electrization of sulphur and phosphorus goes far to prove that they contain combined hydrogen. From the phenomena of the action of potassium upon them in my first experiments, I conceived that they contained oxygen, though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxygen, till I discovered that similar phenomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hydrogen, and on phosphuretted hydrogen, I find that the phenomena differ very much according to the circumstances of the experiment, and in some instances I have obtained a larger volume of gas from potassium after it had been exposed to the action of certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the society. The idea of the existence of oxygen in sulphur and



cion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter, to acid matter, might be supposed capable of being ascertained by their relations to oxygen, or by the peculiar state of their electrical energy.

The whole tenor of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phenomena with at least equal facility.

If hydrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxygen, hydrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases, with hydrogen; the earths, the fixed alkalis, metallic oxides, and the common acids, will be compounds of the same bases, with water.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are: First, the properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hydrogen or oxygen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hydrogen, whether in its simple form, or in combination with one proportion of oxygen, or a double proportion, from my experiments seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hyduret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

Second, the facility with which metallic substances are revived, in cases in which hydrogen is present. I placed two plati-

phosphorus is, however, still supported by various analogies. Their being nonconductors of electricity is one argument in favour of this. Potassium and sodium, I find, when heated in hydrogen, mixed with a small quantity of atmospheric air, absorb both oxygen and hydrogen, and become nonconducting inflammable bodies, analogous to resinous and oily substances.

na wires, positively and negatively electrified from 500 double plates of six inches, in fused litharge: there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed: the difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hydrogen is essential to the production of the metal.

Third, oxygen and hydrogen are bodies that in all cases seem to neutralize each other, and therefore in the products of combustion it might be expected that the natural energies of the bases would be most distinctly displayed, which is the case; and in oxymuriatic acid, the acid energy seems to be blunted by oxygen, and is restored by the addition of hydrogen.

In the action of potassium and sodium upon ammonia, though the quantity of hydrogen evolved in my experiments is not exactly the same as that produced by their action upon water; yet it is probable that this is caused by the imperfection of the process;\* and supposing potassium and sodium to produce the same quantity of hydrogen from ammonia and water, the circumstance, at first view, may be conceived favourable to the notion that they contain hydrogen, which under common circumstances of combination will be repellant to matter of the same kind: but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then whenever bodies known to contain hydrogen are decomposed by a metal, the quantities of hydrogen ought to be the same, or multiples of each other. Thus, in the de-

\* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hydrogen evolved; *i. e.* whenever the metals of the alkalis act upon ammonia, supposing this body to be composed of three of hydrogen, and one of nitrogen, in volume, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalis and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.



composition of ammonia by potassium and sodium, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is given off; and in the action of water on potassium to form potash, the same quantity of hydrogen ought to be expelled. From my analysis\* of sulphuretted hydrogen, it would appear, that if potassium in forming a combination with this substance sets free hydrogen, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogen, will liberate the same proportion of hydrogen as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hydrogen, because, in acting upon different combinations containing hydrogen, they produce the evolution of equal proportions of this gas, then it might be proved that almost any

\* The composition may be deduced from the experiments in the last Bakerian lecture, which show that it contains a volume of hydrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2.27 of hydrogen, and 33.73 of sulphur. When sulphuretted hydrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hydrogen, it was found that when these gases were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common state; the phosphorus was dark-coloured, and the arsenic appeared as a brown powder, both were probably hydurets: this is confirmed likewise by the action of potassium upon arseniuretted and phosphuretted hydrogen; when the metal is in smaller quantity than is sufficient to decompose the whole of the gases, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogen contain in equal volumes, more hydrogen than sulphuretted hydrogen, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogen, it would appear that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

Potassium, sodium, iron, mercury, and all metals that I have experimented upon, in acting upon muriatic acid gas, evolve the same quantity of hydrogen, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hydrogen in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygen, which may be supposed to exist in hydrogen, that might be applied to explain the composition of nitrogen from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenor of our chemical reasonings.

I shall not at present occupy the time of the society by entering any further into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined to enable us to decide upon their nature, and their relations, or to form any general theory concerning them which is likely to be permanent.



*Explanation of the Figures.*

Fig. 1. The apparatus for electrizing potassium in gases. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water, out of the contact of air, page 148. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a, d, c*, the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of 1000*l.* has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has not been put into action; but reasoning from the effects of that part of it which has been used, some important phenomena may be expected from so great an accumulation of electrical power.

*Observations on Albumen, and some other Animal Fluids; with  
Remarks on their Analysis by electrochemical Decomposition.*

BY WILLIAM BRANDE, F. R. S.

Communicated by the Society for the Improvement of Animal Chemistry.\*

*1. Observations on Mucus, and on the Composition of liquid  
Albumen.*

The results obtained from the chemical analysis of the inter-vertebral fluid of the spalus maximus, an account of which is annexed to Mr. Home's paper "On the nature of the inter-vertebral substance in fish and quadrupeds,"† led me to undertake a series of experiments on mucus, in order to examine the properties of that secretion in its pure state, and to ascertain how far it might be capable of conversion into modifications of gelatine and albumen.

1. Saliva was the first source of mucus to which I directed my attention.

In order to separate the albumen, which Dr. Bostock's analysis has shown it to contain,‡ it was agitated for a short time with an equal quantity of pure water; the solution was then boiled and filtered. I considered the clear fluid, which had passed the filter, as a solution of nearly pure mucus; but found, on applying to it the tests of nitrate of silver, and acetate of lead, that it still contained a very considerable proportion of saline matter. The precipitate consisted of muriate and phosphate of silver and lead, in combination with a little animal matter, the odour of which was perceptible on exposing it to heat after it had been washed and dried.

One thousand grains of saliva afforded, by careful evaporation in a water bath, a residuum weighing one hundred and eighty grains, from which twenty grains of saline matter, consisting of phosphate of lime and muriate of soda, were obtained by incineration.

2. The mucus from the trachea, and that of the oyster were next examined; but here the proportion of saline matter was

\* Philosophical Transactions for 1809, p. 373.

† Philos. Trans. 1809; or Journal, vol. XXV. p. 214.

‡ Nicholson's Journal, vol. XIV. page 149.



greater than in the former case, although no traces of albumen could be detected by the usual tests of heat, alcohol and acids.

Finding, therefore, that the reagents employed to detect mucus\* act principally upon the salts which it contains, and not merely upon the secretion itself, it became an object of some importance to find out a method of depriving it of its saline ingredients, by such means as should not affect the mucus. Decomposition by electricity immediately occurred to me, as the most likely means of attaining the object I had in view.

For this purpose, I procured three glass cups, each capable of holding rather more than a measured half ounce of water; one of these was filled with a mixture of equal parts of saliva and pure water; this was connected with the other two, containing pure water, by filaments of moistened cotton. The water in one of the cups was rendered positive, that in the other negative, by a Voltaic battery of one hundred and twenty four-inch double plates, charged with a solution of nitro-muriatic acid, in the proportion of one part of the mixed acid to thirty parts of water.† By continuing this process, I hoped to decompose the saline ingredients of the saliva, to collect the acid matter in the positive, and the alkaline matter in the negative cup, and thus to leave the mucus and albumen in the centre vessel (free from the salts which they contain in their natural state), and to have separated them by boiling distilled water, which would then have afforded a solution of pure mucus.

When the action of the battery had been continued for about ten minutes, a considerable quantity of a white substance surrounded, and adhered to, the cotton on the negative side of the circuit, whereas on the positive side no such effect had taken place.

I could not at first account for this appearance, conceiving that, if it depended on the coagulation of albumen held in solution in the saliva, it would have taken place at the positive pole, in consequence of the acid there separated.

\* Nitrate of silver and acetate of lead. Vide Thomson's System of Chemistry, Vol. V. page 500, 3d edition; and Nicholson's Journal XI—251.

† It was conceived, that this electrical power, though sufficient for the decomposition of the salts, would not materially affect the animal matter.

To ascertain this point, an experiment was made on the albumen of an egg.

When the conductors from the same battery were brought within two inches of each other in this fluid, an immediate and rapid coagulation took place at the negative wire, while only a thin film of albumen collected at the positive wire, where its appearance was readily accounted for, by the separation of a little acid, which, reacting on the albumen, would render it solid; but the cause of the abundant coagulation at the negative pole was not so obvious.

This result I mentioned to Mr. Davy, who immediately offered an explanation of it, by supposing the fluidity of albumen to depend upon the presence of alkaline matter, the separation of which, at the negative pole, would cause it to assume a solid form. I had only to follow up this idea, and shall proceed to state the principal experiments, which were undertaken to establish so probable an opinion.\*

1. When coagulated albumen, cut into small pieces, is boiled in distilled water, it imparts a viscosity to that fluid, showing that something is retained in solution.

Two hundred grains of the coagulated albumen of an egg were repeatedly washed and triturated in four ounces of distilled water, which was afterwards separated by a filter, and evaporated to about one fourth of its original bulk. It was then examined by the usual tests, and was found evidently alkaline; it converted the yellow of turmeric to a pale brown, and restored the blue colour to litmus paper, reddened by vinegar; but it did not appear to effervesce on the addition of a dilute acid.

On evaporating this alkaline fluid to dryness, by a gentle heat, a viscid substance, soluble in water, was obtained. This solution was rendered slightly turbid by an acid; and by the application of electricity, from sixty four-inch double plates, a copious coagulation took place at the negative pole.

\* On referring afterwards to Dr. Thomson's System of Chemistry, (Vol. V. page 491), I find, that a very similar explanation of the coagulation of albumen has been offered by that author, which the following experiments will likewise confirm.



So that water, in which the coagulated white of egg has been boiled, is in fact an extremely dilute alkaline solution of albumen.

This enables us also to explain why albumen becomes coagulated simply by heat.

When the coagulated white of egg is cut into pieces, a small quantity of a brown viscid fluid gradually separates from it, as has been observed by Dr. Bostock in his paper on the primary animal fluids.\* This I find to consist principally of an alkaline solution of albumen. It reddens turmeric, and coagulates abundantly on the application of negative electricity.

It appears, therefore, that the white of an egg, in its fluid state, is a compound of albumen with alkali and water; that when heat is applied to it, the affinities existing between these bodies are modified; that the alkali, before in chemical combination with the albumen, is transferred to the water, and that this separation causes the coagulation of the albumen: the aqueous alkaline solution, which is thus formed, reacts upon the coagulated albumen, of which it dissolves a small portion, and then appears in the form of the brown viscid fluid already noticed.

The coagulation of albumen by alcohol and by acids may be explained by a reference to the principles already laid down.

1. Five hundred grains of the white of egg were agitated with two ounces of pure alcohol; an immediate coagulation resulted, which was rendered more perfect by the application of a very gentle heat. The liquor was separated from the coagulum by filtration, and evaporated to half its bulk; when the usual tests were now applied, alkaline matter was abundantly indicated.

In this instance then, the albumen, in passing from the liquid to the solid state, gives its alkali to the alcohol.†

2. When acids are applied to albumen, these effect its coagulation from the same cause: they render it more rapidly and

\* Nicholson's Journal, Vol. XI—246.

† When albumen is coagulated by alcohol, it does not become so perfectly solid as in most other instances, because the separation effected by the relative affinities is not so complete.

more perfectly solid, on account of their superior affinity for the alkali.

The following experiments were instituted with a view to ascertain the nature and quantity of the alkaline matter which exists in liquid albumen.

1. Five hundred grains of the liquid white of egg were mixed with two ounces of distilled water, and exposed for half an hour to a temperature of  $212^{\circ}$ . The fluid was then separated by a filter, and the coagulated albumen cut into small pieces, and repeatedly washed with boiling distilled water. The filtrated fluid was evaporated to half an ounce by measure; it had a saline taste, it was somewhat turbid, and slightly alkaline; on cooling, it gradually deposited a few flakes of albumen: it was electrified positively in a small glass cup, connected by washed cotton to another similar vessel containing a little distilled water, negatively electrified by one hundred four-inch plates, charged with a solution of nitro-muriatic acid of the same strength as that employed in a former experiment, fresh portions of water being occasionally added in order to compensate for the loss by its decomposition.

When the electrization had been carried on in this way for one hour the cups were removed, and their contents examined.

The fluid in the negatively electrified cup acted rapidly on turmeric, rendering it deep brown. On evaporation and subsequent exposure to a low red heat, it afforded a residuum weighing 5.5 grains, which had the properties of soda, in a state approaching to purity.

The positive cup contained a little coagulated albumen, and an acid which was principally, if not entirely the muriatic, was held in solution by the water: it gave a very copious precipitate with nitrate of silver, which became speedily black on exposure to light. When saturated with carbonate of soda, and evaporated, it afforded a salt in small cubic crystals, from which the fumes of muriatic acid were developed by the action of the sulphuric.

This experiment shows, that, exclusive of soda in an uncombined state, fluid albumen contains some muriate of soda.\*

\* May not a submuriate of soda exist in fluid albumen?



We learn from the experiments of Mr. Hatchett, that minute quantities of other saline bodies are likewise present.\*

In the foregoing experiments, I had generally employed from sixty to three hundred four-inch double plates of copper and zinc, but in subsequent researches, made with a view of ascertaining the action of lower powers, the effects of which I shall afterwards relate, I find that a battery of twenty-four three-inch double plates is sufficient to effect a perfect coagulation at the negative pole, even where the albumen is diluted with so large a quantity of water, as not to be detected by the usual tests.

## II. *Observations on the Composition of some animal Fluids containing Albumen.*

Finding, from the experiments detailed in the preceding section, that albumen may exist in such states of combination, as not to be detected by the usual tests, but separable by electrical decomposition, I was induced to apply this mode of analysis to the examination of animal fluids in general.

### 1. *Saliva.*

When saliva is boiled in water, a few flakes of coagulated albumen are deposited; but this is by no means the whole quantity of albumen contained in the secretion, for on applying the test of negative electricity to the filtered fluid obtained after the separation of the albumen by heat, a copious coagulation and separation of alkali are produced at the negative pole. A

\* After the destructive distillation of coagulated, dry, semitransparent albumen, there remained "a spongy coal of very difficult incineration; as "towards the end of the process it appeared vitrified, and glazed with a "melted saline coat, which was, however, easily dissolved by water. The "residuum was again exposed to a long continued red heat, and again treated with water, till, at length, a few scarcely visible particles remained, "which, as far as such small quantity would permit to be ascertained, "proved to be phosphate of lime. The portion dissolved by water (which "was by much the most considerable) consisted principally of carbonate, "mixed with a small quantity of phosphate of soda.

"Five hundred grains of dry albumen afforded 74.50 grains of coal, of "which 11.25 were saline matter."

See "Chemical experiments on Zoophytes, with some Observations on the "Component Parts of Membrane." Phil. Trans. 1800.

large portion of albumen may therefore exist in a fluid, incapable of separation by heat, and in the present instance not to be detected even by acids, these reagents producing no effect on the filtered solution just alluded to.

*2. Mucus of the Oyster.*

The solution of mucus obtained by agitating oysters in water exhibits to the usual tests no traces of albumen; but when acted upon by electricity from the Voltaic battery, a considerable and rapid coagulation takes place at the negatively electrified wire.

*3. Mucus of the Trachea, &c.*

The other varieties of mucus, as from the trachea, the nose, &c., agree with the former, in affording abundance of albumen by electric decomposition; whereas scarcely any traces of this substance can be detected by the tests of acids, heat, or alcohol.

In these experiments, alkaline matter was always evolved at the negative, and acid at the positive wire. Minute researches, made with a view of ascertaining the nature of the alkaline and acid matter thus evolved, showed the former to consist of soda, with traces of lime; the latter of muriatic acid, with traces of phosphoric acid in the cases of saliva, and mucus of the trachea and nose: the mucus of the oyster afforded only soda and muriatic acid.

On examining the proportions of alkali and acid, the former seemed always to predominate, although in the original fluids no traces of uncombined alkali (as in the white of egg) are to be detected.

These results lead to new ideas respecting the composition of mucus: Is it a peculiar combination of muriate of soda and albumen? or may it not be a compound of soda and albumen, in which the alkali is not separable by the usual modes of analysis, but which yields to the superior decomposing energy of electricity?

*4. Bile.*

An immediate coagulation took place in this secretion, at the negative conductor, the albumen being tinged through-



out of a green colour, arising from the colouring matter at the same time separated.

The relative proportion of albumen, separable by electricity from different specimens of ox-bile, was found to be liable to considerable variation, so that a detailed analysis of this fluid cannot be generally depended upon. I have found the albumen in bile to vary in quantity from 0.5 to three per cent.; and it is somewhat remarkable, that where there is a small quantity of albumen, there likewise the proportion of the resinous matter of bile is relatively small.

The electrochemical decomposition of this fluid affords, beside the results just mentioned, a considerable quantity of soda at the negative pole; and at the positive pole, a mixture of muriatic and phosphoric acids.

#### 5. *Milk.*

In this fluid, the separation of albuminous matter at the negative pole is equally evident, though not so rapid, as in most other cases. The conductors from sixty four-inch double plates, highly charged, and immersed within four inches of each other in three ounces of cow's milk, during one hour, produced the appearance of curds and whey, the principal part of the curd being collected in the neighbourhood of the negative wire, and but little at the positive wire. When this experiment was so conducted, as to collect the products in separate vessels, the predominating ingredients in the contents of the negative cup were soda and traces of lime; and in the positively electrified vessel, a mixture of muriatic and phosphoric acids.

After such decomposition of milk, the serum still affords sugar of milk.

#### 6. *The Liquor of the Amnios.*

An opportunity having offered of examining this secretion, from the human subject, in its pure and fresh state, I shall mention the general results of its analysis.

The liquor of the amnios is almost perfectly transparent, but on exposure to air becomes gradually turbid, and deposits a white flaky matter. It renders tincture of violets green, and while perfectly fresh does not affect litmus; but sulphuretted

Hydrogen is soon evolved from it, and then it slightly reddens litmus. When heated, it becomes turbid, and lets fall flakes of coagulated albumen. Acids render it slightly turbid from the same cause.

Alkalis produce no change, unless when added in considerable excess: the odour of ammonia is then perceptible.

Electrical analysis afforded albumen and soda at the negative pole, and muriatic acid at the positive pole. Hence we learn, that the liquor of the amnios has the properties of a dilute solution of liquid albumen.\*

#### 7. Pus.

In the pus of a healthy sore, coagulation took place at both poles; most abundantly, however, at the negative pole. A slight degree of putrefaction having commenced in the pus which was examined, I did not pay particular attention to the other products of the experiment.

In concluding this section, it may be proper to remark, that the decomposition of liquid albumen by Voltaic electricity takes place in different ways, according to the power employed. With a comparatively high electrical power, the coagulation goes on rapidly at the negative pole, and only very slowly at the positive pole; whereas, with an extremely low power, the coagulation is comparatively rapid at the positive surface, an alkaline solution of albumen surrounding the negative pole. Thus, when the conductors from twenty-four four-inch double plates, highly charged, were brought within half an inch of each other, in a dilute solution of albumen (consisting of one part of albumen to six of water), the coagulation was considerably more abundant at the negative than at the positive pole; but when the conductors were removed from each other to a distance of eight inches, or when they remained at half an inch, being connected with a battery of six four-inch double plates only, the coagulation was only perceptible at the positive pole, in consequence of the acid there collected. Hence we may

\*The difference in the results of the analysis given in the text, and that of Vauquelin and Buniva, most probably arises from the liquor of the amnios examined by those chemists not having been perfectly recent, and perhaps mixed with other secretions. See *Annales de Chimie*, XXXIII. p. 270.



infer, that a rapid abstraction of alkali is necessary to the perfect coagulation of albumen, since, in the cases above alluded to, the albumen remains in solution.

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*An Account of the Effects produced by a large quantity of Laudanum taken Internally, and of the Means used to counteract those Effects.*

BY ALEXANDER MARCET, M. D. F. R. S.

One of the Physicians to Guy's Hospital.

From the Medico-Chirurgical Transactions for, 1809.

ON the sixth of November last, Mr. Astley Cooper informed me, at four o'clock in the afternoon, that he had just seen a young man about eighteen years of age, who had taken, at ten o'clock in the morning, no less than *six ounces of laudanum*, the whole of which had remained in his stomach, and had brought on symptoms which appeared to threaten immediate dissolution. Mr. Cooper who did not see him till three o'clock, that is about five hours after the accident, acquainted me that he had made him swallow, at half past three o'clock, a solution of one drachm and a half of white vitriol, or sulphat of zinc, which had produced some nausea, and had made him vomit about one ounce and a half of fluid which had a strong smell of opium; notwithstanding which the lethargy had gradually increased, and he had at last fallen into a state of complete insensibility. Some mustard had also been administered, without any obvious effect.

Mr. Cooper having requested me to see this gentleman, and to take any farther steps which circumstances might suggest, I called upon him a few minutes after four o'clock, when I found him on the floor, resting on his knees with his body leaning forwards and supported by two friends, who, as I afterwards learnt, were in the act of laying him down in order to let him die in peace. His head was hanging lifeless on his breast, with his eyes shut and his countenance ghastly. His

respiration was slow and sonorous, like the apoplectic breathing. His hands were cold, and the pulse beat from ninety to ninety-six strokes in a minute, in a feeble and irregular manner. All the muscles of his body were in a state of extreme relaxation, and the flesh of his arms in particular felt singularly soft and inelastic.

Blue vitriol or sulphat of copper being the first remedy that occurred to me as likely to produce vomiting, about half a drachm of this substance was quickly dissolved in water, and the patient being abruptly raised from the floor and strongly shaken, he opened his eyes, and seemed disposed to offer a kind of feeble resistance to the attempt he saw us about to make. We succeeded, however, in pouring into his throat about half the quantity of vitriolated copper, just mentioned, that is a dose equivalent to fifteen grains, which he swallowed with a kind of agonizing effort. Immediately after this, his countenance, which had been for an instant roused, became still more ghastly. But he had scarcely taken the dose one minute when he suddenly threw up a large quantity of a brownish fluid, which had a strong smell of laudanum, and was immediately followed by two or three more gushes of the same liquid, the whole amounting to between one and two pints. He was then made to swallow some warm water, and was dragged from one room to another, with a view to counteract the state of torpor above described. His limbs at first were quite passive and lifeless, but in a few minutes he began to rest, in some degree, on his legs, with the assistance of his friends. He continued, however, with his eyes closed, (unless roused by a loud and sudden call) his pupils dilated, and his breathing apoplectic. I strongly recommended to his friends, who fortunately were extremely active and intelligent, that he should be kept incessantly on his legs, and in constant motion about his room.

When I called again at nine o'clock in the evening, I found him so far recovered as to walk about the room supported by a friend. His countenance appeared more natural, and he was able, when urged by questions, to answer by monosyllables like a man in a state of extreme intoxication. He had vomited once or twice more since I had left him in the afternoon. He gave me to understand that he felt cold at the pit of his sto-



mach, hot on the surface of his body, and cold in the extremities. In spite of this improvement he still slept profoundly, and snored loudly, even whilst in the act of walking about the room; and when forcibly roused, he opened his eyes for an instant and fell again into a deep slumber. Mr. Cooper saw him also in the evening, and we both agreed in recommending that he should be kept in the same state of forced activity through the night, that he should take frequent doses of assafetida with volatile alkali, of camphor, or even of musk, if other stimulants did not appear sufficiently active. It was also agreed that his head should be blistered and sinapisms applied to his feet. Some tea and coffee, and likewise lemon juice, (of which he had taken small quantities during the evening with very good effect,) were directed to be frequently offered to him, and we strongly recommended that he should never be longer than half an hour at a time during the night, without being roused, in order to take either some medicines or some liquid nourishment.

On calling the next morning, (November 7th) I learnt that he had got so much better by twelve o'clock at night, that his friends had found it unnecessary to apply the blister, and that a few doses of camphorated julep, with assafetida, were the only remedies he had taken. But he had frequently sipped small quantities of tea, coffee, and lemon juice, all of which were extremely grateful to him. He was prevented from going to sleep, and kept in constant agitation by his friends, till six o'clock in the morning, when he was allowed to go to bed.

On the following morning, between nine and ten o'clock, I found him still asleep; but on my approaching his bed, he readily awoke being rather confused at first, but soon recovering himself, he said, he supposed he had slept three or four hours, which was exactly the case, and complained of his throat being sore, as if excoriated. He observed also, that a glyster which had been administered during the night, was coming away by degrees, mixed with fæces, without his being sensible of it, or able to prevent it.

On the following day (November 8,) he was able to take a walk out of doors. His appetite was not yet returned; but he was not averse to taking food. He still complained of soreness

in his throat, and also at the root of his tongue, both of which were evidently the effects of the caustic antidotes he had swallowed. He had had no passage through his body since his illness, except that which was occasioned by the glyster. He still looked sallow and dejected, and complained of an uneasy sensation at the pit of his stomach, not, however, amounting to pain. I advised him to take a dose of rhubarb and calomel.

In a few days after this he was perfectly recovered. The circumstances which occasioned this accident having no sort of connexion with the symptoms which it produced, I have thought it useless to enter into a detailed account of them. But I should not omit to observe, that after a strict inquiry into all particulars, it was perfectly ascertained that the quantity of laudanum taken was actually six ounces.

The inference which may naturally be drawn from this case, and which, indeed, induced me to consider it as sufficiently important to be communicated to the Society, is, that neither the lapse of several hours after an accident of this kind, nor the failure of various means of relief, should prevent the administration and diligent repetition of the most powerful emetics.

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*A Case of Artificial dilatation of the Female Urethra, &c. &c.*

BY H. L. THOMAS, ESQ. F. R. S.

From the Medico-Chirurgical Transactions for 1809.

SUNDAY, April 26th, I was called into the country to see a lady, thirty-four years of age, and the mother of several children. On the morning of that day she had, without any assignable cause, laboured under a suppression of urine; as there was no regular assistance at hand, the husband (whose readings on this subject had been pretty extensive), took upon himself to afford her relief. With this view he introduced an ivory ear-picker into the meatus urinarius, and the water immediately flowed, but as the quantity evacuated did not quite equal his expectations, the instrument was introduced a second time,



when it escaped from his fingers, and suddenly slipped, as he expressed it, with a "jerk into the body." When I saw her, six hours after, she was free from pain, and with no other unpleasant symptom than those which might be expected from the agitated state of her mind. Upon introducing the sound the extraneous substance was readily detected, and was as easily laid hold of by a pair of fine polypus forceps, but I found every attempt to extract it was attended with considerable pain, and followed by a slight discharge of blood, appearances which gave me reason to imagine that the instrument lay across the bladder, with its pointed extremities entangled between the fasciculi of muscular fibres, and that much mischief might be produced by any farther attempts at extraction. I therefore merely ordered an enema to be administered, and directed her to drink plentifully of diluting mucilaginous liquids; I also gave some directions respecting the position of the body at the time of evacuating the urine, which were to be such as to render the orifice of the bladder the most depending part.

On the Monday at two o'clock, I found that she had passed a restless night, and was much indisposed; pulse frequent, face flushed, and the tongue furred; a slight degree of tenderness was beginning to pervade the whole of the hypogastric region. This situation of the patient clearly pointed out the propriety of a speedy removal of the cause of irritation; and as I came provided with the *bistoiré cachée*, I proposed opening into the bladder immediately; this proposition was, however, absolutely rejected by the husband, from the knowledge he had of a case of lithotomy in a female, who ever afterwards suffered from an incontinency of urine.

With some hesitation, and I must confess with little prospect of success, I introduced a piece of sponge tent into the urethra, three inches long, and somewhat thicker than the full sized female sound, leaving directions for its removal at the end of two hours, or even sooner, if attended with much pain or distress. I took care to prevent the possibility of the sponge separating, and any part escaping into the bladder, by passing a strong string through its whole length secured at both extremities by a double knot.

In the evening I was informed that she had borne the tent for two hours, without any great degree of irritation taking place, and that during the whole time the urine gradually escaped. I sent another piece of the tent somewhat larger than the first, desiring it to be introduced at twelve o'clock the following morning, and allowed to remain in the urethra till I came to her at two. On my arrival I found her tolerably well, and upon withdrawing the tent I passed the fore finger of my left hand into the bladder, where I felt the ear-picker lying across the cervix. At first I conceived it impossible, without laceration to disengage it from the position in which it appeared so firmly fixed, but by turning my hand, and insinuating the point of the fore finger underneath, and towards the blunt extremity, it was readily dislodged, and escaped through the urethra, by the side of my finger. It measured three inches in length, one end being considerably more pointed than the other. I kept my finger engaged in the bladder for (I should guess) five minutes after the expulsion of the ear-picker, solely with a view of attending to the contractile force of that viscus. The sensations I experienced did not in any way accord with the ideas I had formed, for instead of my finger being firmly compressed, as I expected, by the sphincter muscle and bladder generally, I found every where nothing else than a soft pulpy yielding substance, totally insensible to any stimulus I could produce by the finger nail; this inability to contract, may perhaps be accounted for by the partial distention of the bladder for so long a time by the ear-picker. The same agent, however, cannot be assigned as the cause of the general relaxation which had taken place in the urethra, and which was so very complete, that before I withdrew my finger, I believe, had the case required it, both thumb and finger would have passed into the bladder without the smallest difficulty.

On the following day the patient was so well as to preclude even an excuse for any farther examination of the parts; but I was given to understand that the involuntary discharge of urine continued only six hours, and that now she possessed the full powers of expelling it without any other inconvenience than a slight degree of scalding in its passage along the urethra.



My view in laying this case before the Society is to show how readily, and with what little pain the female urethra will admit of dilatation. There is no novelty in the mode of accomplishing the object of distention. Near a century back it was proposed by Douglas; he not only recommended sponge for this purpose, but also dried gentian root, as being more gradual in its expansion, and better fitted for the purpose.

Mr. Bloomfield has given the case of a young girl where he effected the dilatation by introducing the cœcum of a small animal, in a collapsed state, into the bladder, filling it afterwards with warm water, by means of a syringe. This was gradually withdrawn as the cervix vesicæ opened, and in a few hours the dilatation was so far accomplished as to allow the calculus to pass through.

Why some of these methods have not been more generally adopted I cannot say, perhaps the incontinency of urine which occasionally has been observed to succeed to great distention of the urethra is the reason of their having been laid aside; with what propriety, however, may be questioned, for I believe it will be found that this unpleasant symptom as frequently occurs after the operation of lithotomy, as it is now usually performed.

We have many well authenticated cases on record where calculi, of a size larger than a hen's egg, have been expelled from the bladder by the expulsive efforts of its own muscles. Heister has related from good authority several instances of the kind, a very remarkable case is also given by Dr. Molineux, in the early part of the Philosophical Transactions, where a stone was voided by a woman, "the circumference of which measured the longest way  $7\frac{6}{10}$  inches, and round about where "it was thickest  $5\frac{3}{4}$  inches, its weight near  $2\frac{1}{2}$  ounces troy."

If these relations can be credited, and there is no reason why they should not, I can hardly conceive any cause in a young and healthy female subject, and where the bladder is free from disease, why a very large stone may not be extracted without the use of any other instrument than the forceps, the urethra having first been sufficiently dilated by means of the sponge tents; for this purpose the blades of the forceps need not be so thick and strong as those commonly employed.

It may be often observed, that muscular fibres, when once stretched to a certain extent, (i. e.) beyond their capability of immediately reacting, will admit of very considerable elongation without tearing, and yet shall, when the cause is removed, perfectly and very speedily recover their original powers. When the luxation of the femur takes place downwards, we observe the great gluteus muscle upon the stretch, almost to breaking: before the reduction can be accomplished the fibres will be still farther elongated, yet after this violence of extension, laceration of the fibres hardly ever happens, and when the muscles are again restored to their former situation, their functions become as perfect in a few days as if no injury had been sustained. That sphincter muscles are possessed of similar powers, the above case will, I think, in great measure prove, in addition to which I beg leave to subjoin the following.

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A gentleman of an inactive and sedentary disposition had for many years suffered from constipated bowels, which increased to that degree that the most active cathartics failed in producing the desired effect. By the advice of a practitioner, whom he consulted in Paris, he daily introduced into the rectum a piece of flexible cane (about a finger's thickness), where it was allowed to remain until the desire for evacuating the *fæces* came on. This plan succeeded so well that for more than a twelve month he never had occasion to resort to any other means. One morning, being anxious to fulfil a particular engagement in good time, in his hurry he passed the stick farther up, and with less caution than usual, when it was suddenly sucked up into the body, beyond the reach of his fingers. This accident, however, did not interrupt the free discharge of the *fæces*, and the same evacuation regularly took place every day, whilst the stick remained in the gut. It was seven days afterwards when I first saw him; he was in a very distressed state, with every symptom of fever, tension of the abdomen, and a countenance expressive of the greatest anxiety. His relatives and friends were totally ignorant of the real nature of his case; and nothing less than the urgency of his sufferings, could ever



have prevailed upon him to disclose it to me. Such were his feelings on the occasion, that a violent hysteric fit was brought on by the mere recital of what he termed his folly.

Upon examination with my finger, per anum, no part of the cane could be discovered; but one end of it was readily felt projecting (as it were) through the parietes of the abdomen, midway between the ilium and the umbilicus on the right side. The slightest pressure upon this part gave him exquisite pain.

After repeated trials I was at length enabled, with a bougie to feel one extremity of the stick lodged high up in the rectum; but without being able to lay hold of it with the stone forceps. To allay the irritation for the present, an emollient clyster, with tinct. opii.  $\mathfrak{z}\text{ij}$ , was given, which passed without the least impediment, and did not return. On the next examination, two hours after, I found the sphincter ani considerably dilated, and by a continued perseverance to increase it, the relaxation became so complete, that in about twenty minutes I was enabled to introduce one finger after the other, until the whole hand was engaged in the rectum.

I found the end of the stick jammed in the hollow of the sacrum, but by bending the body forward it was readily disengaged, and extracted. Its length was nine inches and a half, with one extremity very ragged and uneven.

For several days after, the situation of the patient was highly critical, the local injury, joined to the perturbation of his mind, brought on symptoms truly alarming. At length I had the satisfaction to witness his complete recovery; and he has ever since (more than two years ago) enjoyed good health, and the regular action of the bowels, without the assistance of medicines, or any other aid.

*Observations on the Digestion of the Stomach after Death.*

BY ALLAN BURNS,

Member of the Royal College of Surgeons in London, and Lecturer on  
Anatomy and Surgery in Glasgow.

From the Edinburgh Medical and Surgical Journal for 1810.

WHILE inspecting dead bodies, the stomach has repeatedly been found eroded, in subjects where the previous symptoms gave no reason to expect such an occurrence. Yet, till lately, appearances were ascribed by those who saw them to disease; and the cases were described with peculiar care, because they were thought to illustrate the extensive disorganization which might take place, without being accompanied by characteristic symptoms, and without causing death. This is hardly to be wondered at, when we remember, that our forefathers entertained mistaken, and too frequently preposterous, notions regarding the functions of the stomach. They knew that the food was digested in this viscus; but instead of investigating the laws which regulate this process, they wasted their time in idle speculations, about the supposititious effects of fermentation, trituration, and putrefaction; the agency of the gastric juice was by them completely neglected. Mr. John Hunter, and some enlightened physiologists, his contemporaries, discarding the influence of imaginary causes, began to examine the digestive process. In the course of this inquiry, new facts presented themselves; the stomach was found, not only to afford a lodgment to the food, while the process of digestion was going on, but also it was discovered, that, either by its own vessels, or by those at the termination of the œsophagus, a peculiar fluid was secreted, a fluid possessed of the curious property of dissolving the food, and reducing it to a pultaceous mass. This was a material improvement in the history of the function of digestion; it was a new fact, which the comprehensive mind of Mr. Hunter soon saw might be advantageously employed in illustration of several of the phenomena observed during dissection. He especially applied it to the explanation of some of the cases of erosion of the stomach, and he satisfactorily de-



monstrated, that generally this was produced by agents acting after death. He likewise proved, beyond a doubt, that during life the stomach resists the action of the fluid which dissolves the food; but he, at the same time, made it plainly appear, that the gastric juice is, after the death of the animal, capable of dissolving the stomach which formed it, and which had, during life, resisted its action by its vital principle. Dr. Adams imagines, that Mr. Hunter believed that it was only under certain circumstances of death that this digestion of the stomach would actually happen. "But, for this purpose, he saw that the animal must be in health immediately before death, otherwise, neither the quantity nor quality of the secretion would be equal to the purpose. He was confirmed in this by the instances in which he saw the stomach digested. Both were men who had died from a violent death; both had been previously in sufficient health to eat a hearty meal. The fair inference from these was, that when men die of disease, the appetite usually ceases, and probably the secretion of the gastric juice also."\* Dr. Adams will, I trust, excuse me, when I point out an error he has committed. Mr. Hunter, in his observations, details the history of three examples in which the stomach was digested. Two of the men died shortly after having had their skulls fractured, and the third was "a man who had been hanged."† Dr. Adams also appears to me to have drawn an inference from the two cases which he does notice, by no means warranted by the general tenor of Mr. Hunter's essay. Although the individual cases which Mr. Hunter adduces to prove the fact of the solution of the stomach after death, were subjects who had been deprived of life by violence; still, in no part of his paper, does he limit the action of the gastric juice on the stomach, to such as have been killed by violence. Indeed, it is so much the reverse, that he inculcates, that there are few dead bodies in which the stomach is not in some measure dissolved. Nay, he declares, "that there is frequently a considerable aperture made in this viscus, and, in many subjects, this digestive power extends much farther than through the stomach. I have often

\* Adams's *Observations on Morbid Poisons*, 2d edition, p. 30.

† Hunter's *Observations on Digestion*, p. 185.

found that, after it had dissolved the stomach at the usual place, the contents of the stomach had come into contact with the spleen and diaphragm, had partly dissolved the adjacent side of the spleen, and had dissolved the diaphragm quite through, so that the contents of the stomach were found in the cavity of the thorax, and had even affected the lungs in a small degree."\* I would here beg leave to inquire, is it probable, that one so accurate in his statement, and so faithful in his description, as Mr. Hunter must be allowed, even by his opponents, to have been, would have used such expressions had he merely seen the three cases which he particularizes. It is quite incredible, nor can I conceive how Dr. Adams came to fall into such an error. I have from Mr. Hunter's own paper proved, that he had really seen subjects having the stomach digested, besides these he has specified; but, as these cases were the first which came under his observation, they seem to have made the most lasting impression. From this circumstance, and from the manner in which Mr. Hunter wrote, many have embraced the opinion adopted by Dr. Adams, that solution of the coats of the stomach never does take place, except where the person has been suddenly deprived of life. Yet I do not conceive that Mr. Hunter either sanctions this opinion, or that he was the one who brought it into notice. We find nothing of this kind in his essay, where it is clearly stated, that he had met with digestion in the stomach in some who had died a natural death. Can any thing be more explicit than the statement which Mr. Hunter gives, when he says, that for some time he was at a loss how to explain these solutions of the stomach, especially as "they were most frequent in those who had died of violent deaths." No man will believe that Mr. Hunter would have expressed himself in this way, if he had never met with an instance of digestion of the stomach, except after death from violence. Were this point merely of a speculative nature, and were its decision merely a matter of curiosity, I would consider the time employed in discussing it, as little better than misspent, but when I consider, that it is one on which the practitioner may be called to give his opinion, in a situation where that opi-

\* Hunter's Observations, p. 186.



nion may determine the fate of a criminal, I cannot but view it as an important question, which it is the duty of every one to elucidate to the utmost of his ability. This consideration, and having had some opportunity of investigating this subject, has induced me to collect the observations which I had made during the dissection of those bodies in which I found the stomach digested. These observations have led me to conclude, that digestion of the stomach after death, is neither so rare in its occurrence as some have imagined, nor is it confined to such subjects as had been, previous to death, in a healthy condition; and they have also demonstrated, that other parts of the stomach, besides the large end, may be acted on by the gastric juice.

That the digestion of the coats of the stomach after death is not a very rare occurrence, I think myself authorized to infer, from my having examined nine bodies in which the solution had proceeded to such an extent as to have made holes of considerable size through that viscus. Besides these nine instances in which the digestion of part of the stomach was complete, I have had occasion to see, in opening this viscus, various degrees of dissolution of its villous coat.\* In some of these cases the appearances presented were similar to those described by Mr. Hunter, but in others there were considerable differences. In the bodies examined by Mr. H. "the parts acted on appear smooth, thin and more transparent, and the vessels will be seen ramifying in its substance; and, upon squeezing the blood from the larger to the smaller branches, it will be found to pass out at the digested ends, and appear like drops on its inner surface."† This condition of the vessels, which by Mr. H. is made a prominent feature in those cases where the digestion of the stomach has been produced by the gastric juice, does not invariably accompany such solution. In three of the subjects which I have dissected, there was no appearance of vessels ramifying

\* It will generally be found that, where the coats of the stomach are softened by the gastric juice, the vessels are unable to resist the force of the syringe in injecting the body. In such subjects we therefore find the cavity of the stomach filled with wax, and we likewise see masses of it collected between the coats of the viscus.

† Hunter's Observations, p. 186.

on the coats of the stomach. In accounting for the want of these vessels, some have given one explanation, while others have attempted a different one. To me it has appeared, that time has been wasted on this inquiry. We are not to look for an explanation of this difference from Mr. Hunter's description, as arising from the particular part of the stomach acted on in the different cases; neither are we to imagine that the stage of the process at which we examine the body will assist us in this investigation.\* We are rather to obtain an explanation of this fact, from contemplating the differences of condition of the different individuals at the time of death. Were not the subjects whose cases Mr. Hunter has detailed, men cut off by violence, in the fulness of health, their stomachs, at the time, excited by the stimulus of food to vigorous action, and the process of digestion at the instant of death going on briskly, circumstances under which it is reasonable to infer that all the bloodvessels would be filled with blood, which it is evident, from the nature of the causes depriving them of life, would be detained in the veins? This being the state of his subjects at the moment of death, we shall not wonder that, when he afterwards opened the bodies, he could squeeze the blood from the digested ends of the vessels. In three of my subjects in whom I found the stomach digested, and this ap-

\* It has been supposed, that the appearance of the vessels described by Mr. Hunter, will only be met with in solution of the great end of the stomach. At this point the stomach receives the vasa brevia from the splenic vessels, consequently the bloodvessels are here both larger and more numerous than elsewhere, and hence, when this viscus is dissolved, they are found in the state noticed by Mr. Hunter.

Others again have contended, that the condition of the bloodvessels does not depend on the part of the stomach acted on, but is solely regulated by the stage of solution at which we inspect the parts. If, say they, we examine the stomach, when its inner coat has chiefly been acted on, the vessels will be seen ramifying on those coats which are only partially affected, and blood may be squeezed from the ends of the vessels; but if a complete perforation have been made, we can neither see vessels ramifying on the dissolved part, nor force blood from their ends. Now, I believe it will be found that neither of those afford a satisfactory explanation of the point in question; but as these opinions are contained in papers published regarding a late case which occurred in Liverpool, into a discussion of the merits of which I do not wish to enter, I forbear to make any further comments on them.



pearance of the vessels wanting, the condition of the individuals at the time of death was very different from what it had been in Mr. Hunter's cases. In the former, the people were worn out by debilitating diseases; they were emaciated and anasarctous, with hardly any blood in their vessels, and not a great deal even in their hearts. I was not therefore surprised, when I found no blood in the gastric vessels of these subjects. It would indeed have been more astonishing had these been found containing blood, while the vessels of all the neighbouring viscera were empty. I have with confidence ascribed the solution of the coats of the stomach, in these cases, to the gastric juice, and in proof shall hereafter mention the effect produced on those parts in contact with the hole in the stomach. The facts which have been mentioned show, I think, that digestion of the coats of the stomach may take place under two very different conditions of the body. They prove, that although such solution is most frequent in those who have been suddenly deprived of life when in full health, that it is not confined to those alone, but does, under certain circumstances, with which perhaps we may never become acquainted, occur in those who have died from lingering and debilitating diseases. This last position I bring forward supported by complete proof, without which I would not have ventured to make it public, since it is so opposite to the opinion adopted by many whose authority on such subjects is deservedly high. Perhaps this idea may have originally arisen from the circumstance of Mr. Hunter not having been so explicit as we could have wished he had been. Be this, however, as it may, the opinion is at present so prevalent, that some practitioners of eminent abilities who have lately written on this subject, have asserted, that they can hardly conceive it possible that the stomach can be dissolved by the gastric juice in a person in a state of debility at the time of death. Nor is this the only mistaken notion which seems to be at present entertained on this subject. Mr. Hunter taught, that digestion of the stomach after death was occasioned by that portion of the gastric juice contained in the cavity of the stomach; consequently it followed, as a fair inference from this doctrine, that the coats of this viscus will only be acted on at that part on which the contents of the stomach rested. In Mr. Hunter's

cases, the great end of the stomach, which in the supine position of the body is the most depending part of this viscus, was found to be chiefly affected; a fact which tended to corroborate and support his opinion, and to render extremely probable his conjecture. But this assertion, coming from Mr. Hunter, had a bad effect; for when practitioners met with instances of solution of other parts of the stomach than the great end, they applied to his essay for information on this point; but finding nothing there to render it even probable that such solution might be effected by the gastric juice, other causes were sought for. Poison has been called in to account for the appearance; and thus the life of the person supposed to have administered the deleterious substance might, it is evident, be brought into danger. As Mr. Hunter was seldom in the habit of drawing inferences from an insufficient number of facts, any doctrine broached by him must be rejected with extreme reluctance, and not until we have satisfactory proof of its fallacy. In the present case, Mr. Hunter has surely deviated from his usual caution, in forming the opinion that the great end of the stomach will be the only part of the stomach acted on. The following history will show that he had. About ten months ago I had occasion, two days after death, to open the body of a very emaciated and anasarctous young girl, who had died from scrofulous enlargement of the mesenteric glands. On raising the coverings of the abdomen, the stomach, which was empty, presented itself to view, with its front dissolved. The aperture was of an oblong shape, about two inches in its long diameter, and an inch in its short, with tender, flocculent, and pulpy edges. This I demonstrated to the pupils attending my class; and I especially called their attention to the fact, that the liver, which was in contact with the hole, had no impression made on it. Having proceeded thus far, I placed all the parts as they had been, stitched up the abdomen, and laid the body aside in a cold situation for two days. Then I opened it again, in presence of the same gentlemen, and we found that now the liver, where it lay over the dissolved part of the stomach, was pulpy; its peritoneal coat was completely dissolved, and its substance was tender to a considerable depth. At this time the other parts of the liver were equally solid as before; and as yet every



part of the subject was free from putrefaction. The posterior face of the stomach, opposite to the hole, was dissolved, all except the peritoneal coat; at least the internal coats were rendered pulpy and glutinous. The peritoneal covering had become spongy and more transparent than it ought to have been. There was no blood contained in either the vessels of the stomach, or other abdominal viscera. The appearance of the dissolved part of the stomach, and the digestion of that part of the liver which was over the hole, will be sufficient to prove that the solution, in this case, was really produced by the gastric juice. This being certain, I may be permitted to mention, that the situation of the aperture was different from what it had been in any of Mr. Hunter's cases. It was seated on the fore part of the stomach, about an inch distant from the pylorus, and midway between the smaller and greater curvatures of this viscus, at a part of the stomach with which the gastric juice could not have come into contact, as the body had constantly been in the supine posture.

The same fact I have seen exemplified in other two subjects; but the particulars of these cases I do not consider it necessary to detail, after the full account I have given above.

If, then, the stomach was not acted on by fluid contained in its cavity, how came it to be dissolved? To me it appears, that we cannot, with propriety, ascribe the digestion of the stomach, in every case, to the gastric juice which has been poured into the cavity of that viscus. We are more properly to refer it, in some instances, to the action of the gastric fluid retained in the vessels which had secreted it. If this be admitted to be a correct explanation of the fact, that the fore part of the stomach is sometimes digested by the gastric juice, we shall cease to have any difficulty in accounting for the dissolution of other parts of this viscus, besides the large end. We shall learn, that the part acted on must vary, according to the place of the stomach where the gastric juice is retained in the apparatus which secreted it, and thus we shall be enabled to explain some cases, which, at present, seem to be in opposition to the observations of Mr. Hunter.

I cannot finish these remarks, without calling the attention to one point noticed in the history of the case: I allude to the state

of the neighbouring viscera, at the opening of the body, two days after death. At that time, none of them were in any degree affected; yet it has been seen, that when the subject had been allowed to lie for other two days, that part of the liver, in contact with the dissolved portion of the stomach, was also in a state of solution. At this stage, the coats of the stomach opposite to the hole were likewise acted on. It is, therefore, to be remembered, in judicial investigations into the cause of dissolution of the coats of this viscus, that the appearances will vary, according to the period after death at which the body is examined.

The opinion, of the digestion of the stomach being produced by that portion of the gastric juice contained in the cavity of this viscus, will be found to be enforced by Mr. Everard Home, in his paper "on the structure and uses of the spleen." Mr. Home supposes, that the human stomach is divided by a muscular contraction, into a cardiac and pyloric portion; and he further believes, that, in the first, the food is acted on by the gastric juice. It is there that it is reduced to the pultaceous mass, which afterward is propelled into the pyloric cyst, where the chyle is separated from it. "That the food is dissolved in the cardiac portion of the human stomach, is proved, by that part *only* being found digested after death; the instances of which are sufficiently numerous to require no addition being made to them. This could not take place, unless the solvent liquor was deposited there." I do not pretend to decide on the propriety of Mr. Home's opinion, regarding the part of the stomach in which the food is digested, but I think the cases which I have mentioned go a considerable way to disprove it. But here, I must point out the fallacy of one point of his statement. Have we not seen that the fore part of the pyloric end of the stomach has actually been found digested after death? and having seen this, are we not, equally with Mr. Home, authorized to infer, that the food is alike digested in the pyloric, as in the cardiac portion of this viscus. It may be said, that as the stomach was not, in these cases where its pyloric end was dissolved, divided into two cavities, the gastric juice, which was originally deposited in the cardiac portion, might, in moving the body, have passed into the pyloric part, and thus dissolved its coats. The



case which I have detailed at length will refute this idea. In it, the solution took place at a part of the stomach where it could not have occurred, had it been occasioned by fluid lodged in its cavity. While I thus object to one part of Mr. Home's paper, it is but justice to add, that I often have found the stomach, as he describes, divided by a muscular contraction into cardiac and pyloric cysts.

It may, perhaps, be expected, that I should now assign a reason, why the solution takes place in some subjects and not in others. I am not ignorant of the explanation which Dr. Adams has given of this fact, yet, I know so little concerning this part of the subject, that I feel myself neither warranted to adopt or reject his explanation of it. I do not know whether, as Dr. Adams believes, it depends on the suddenness of actual death, as put in opposition to apparent death, or, whether it arises from some alteration in the quality of the gastric juice. At all events, I consider it sufficient for my purpose, to have pointed out the facts which I have detailed, regarding the digestion of the stomach. I leave it therefore to others, to explain the immediate circumstances, rendering the stomach liable to be dissolved.

As in some degree connected with the accounts given of the digestion of the stomach after death, I may mention, that, in four instances, I have found every part of the alimentary canal, from the cardiac orifice of the stomach, down to the beginning of the rectum, dissolved into a pulpy glutinous mass, transparent, and bearing some resemblance to thick starch. Not a single point of either the stomach or intestinal tube, but was so much acted on, that it tore whenever it was even gently touched. The other viscera were not acted on. The subjects were young children, fat, and free from putrefaction, and in whose bodies I could distinguish nothingsatisfactory to account for death. In all of them, the abdomen, when opened, emitted a sour smell. As I never met with an instance of this uniform solution of the bowels in a body, with the history of which I had been acquainted previous to death, I do not wish to enter more fully, for the present, into the history of the cases which I have examined. I may, however, add, that I never observed an occurrence of this kind except during the summer months.

*Glasgow, January 1, 1810.*

## SELECTED REVIEWS.

*An Essay on Hydrocephalus Acutus; or, Dropsy of the Brain.*  
By John Cheyne, M. D. Edin. 1808. 8vo. pp. 218.

From the Edinburgh Medical and Surgical Journal for 1808.

ALTHOUGH the more prominent and characteristic features of hydrocephalus acutus have been fully and accurately described by former writers, the history of the disease still remained, in many respects, imperfect. Its antecedents, and even the symptoms which often mark its insidious and gradual approach, and earlier progress, had not been well observed. The occasional causes of the disease have been mistaken, or overrated, and very false notions of its pathology have been entertained. Hence the disease has been considered either as a dropsy, or as a phrenitic affection of the brain; and the method of treatment has been consequently vacillating and uncertain, and every one knows but too well how unsuccessful. The object of the essay before us is to supply some of these defects; and we have no hesitation in saying, that Dr. Cheyne has added considerably to the facts which serve to complete the history of this interesting and fatal disease. Some of his pathological observations also are important; but the general view of the pathology of this disease seems still embarrassed and imperfect, and the treatment of the disease, once formed, still where it was. But, as is well observed by the ingenious author, "we are only in the stage of collection, and not advanced, as yet, to that of theory." It is, then, on the very excellent history of the disease, on the numerous and well related clinical cases, and the results inferred from these, that the merits of this performance must rest. But we would more particularly direct our readers' attention to the various modes of attack which distinguish the incipient forms of hydrocephalus, and to the connexion between the hydrocephalic symptoms, and the disordered state of the hepatic and chylopoietic viscera. Of the incipient forms of hydrocephalus acutus, Dr. Cheyne has distinguished three.—In the first, before any characteristic symptoms of the disease appear,



—“The child, for some days, or even weeks, has complained of pains in his head or belly, while, at the same time, he has been slightly feverish, dull, ill complexioned, without appetite, or, perhaps, with an increased appetite, and with considerable disorder of all the functions of the abdominal viscera. These complaints arise gradually, but are seldom alarming; and the child's friends are not awakened to a sense of his danger until, advancing a step farther, the commencement of a peculiar disease has more distinctly shown itself. The dullness and severe pains in his head are now accompanied, perhaps, upon getting up in the morning, or after he has begun to stir about, with vomiting. Yet even this symptom is often disregarded until the second or third day of its recurrence, and the disease has made considerable progress before the illness of the patient is suspected to arise from a disordered condition of the brain. When the attention is more particularly excited by these symptoms, the headache (chiefly in the forehead) will be found to return at shorter intervals. The child often affectingly complains of his head. He sighs frequently, is dull, his head requires to be supported; he complains of weariness in his eyes; the pupils sometimes appear unusually contracted, and he has an aversion to light. His tongue is white, and his belly generally costive; the stools are at first clayey; as the disease advances, they become of a gelatinous consistence, dark green, of a sickly smell, sometimes as dark as tar. The pulse becomes quick, and, at particular times of the day, these symptoms are attended with febrile heat and irritability, and the child complains not only of headache, but of pains in different parts of the body, sometimes astonishingly acute. At one time he complains of pains in his limbs, at another, of pains in his breast, or in the nape of the neck, very often in his bowels; and before the anxiety of his friends can make any preparations to relieve him, the pain is gone, or fled to some other part; at another time, he for a long time lies on his mother's knee, restless and whining, as from dull rheumatic pains. These disorders cannot continue without impairing the child's strength; and accordingly, in ten days or a fortnight, the period usually occupied by the first stage of this attack, he is altered in appearance; in his manner he has be-

come feverish and undecided, his hand tremulous, and his gait tottering."

"In the second form of the attack, the disease runs a more rapid course. This does not occur so frequently, yet it is well intitled to the attention of every one wishing to gain a clear conception of hydrocephalus. After the child has been drooping for a short time, which, although it sometimes escapes observation, is generally recollected, there is a sudden change to a fever, attended, even from the first, with a great degree of pyrexia, with frequent, but short and irregular remissions, flushing, severe headache, tenderness all over the abdomen, and increased sensibility, with sometimes brilliancy of the eyes. It is said to be often difficult immediately to distinguish hydrocephalus from fever; and this is the form of the disease in which there is the greatest resemblance between the two diseases; but we are led to suspect some deeply seated evil, from the frantic screams and complaints of the head and belly, alternately with stupor, or rather lowness; and we are struck with the irritability of the stomach, in a degree beyond what we find in fevers of this country, retching and vomiting being often brought on by a change of posture, certainly by every attempt to sit up in bed; and with the disordered state of the bowels which attends this irritability of the stomach. And when, at any time, the child has a little respite from the violence of these symptoms, we find our suspicion confirmed by his look; for in this disease, when the features do not express pain or terror, there is not unfrequently an expression, which it has in common with some other diseases of the brain, of dejection bordering upon insensibility, which is quite insupportable to those who are interested in his recovery."

The third mode of attack our author has observed less frequently than the first, but more so than the second.

"It may be considered as an instance of that conversion of diseases, which arises either from the excess or combination of the symptoms of the original complaint, operating upon a habit favourable to the new disorder, or perhaps from the state of the habit produced by the primary giving rise to the consequential disease. When hydrocephalus arises after an imperfect state of health, as where there had been a scrofulous action



which has abated; or where from predisposition and the anomalism of the symptoms, such action is looked for; or where the child has had some epidemic disease formerly, (perhaps many months before) from which he has not perfectly recovered, or regained sound health; the attack is sometimes made with all the violence which I have described as distinguishing the second form. When again the attack comes on as the sequel of an acute disease or fever, hooping-cough, perhaps dentition, or during some actually existing scrofulous disease, then the child almost imperceptibly slips into hydrocephalus; there are scarcely any of the acute symptoms; and the palsies or convulsions are the first indication of the new disorder."

We have thought these descriptions of the modes of attack of so much importance, that we have given them in the author's words. It is by attending to these, that the disease can be detected in its earlier stages, and when it is most under the control of art. Our author then goes on to complete the general history of the disease. The three stages marked by Dr. White, as distinguished by the changes of the pulse, he thinks may be more usefully retained under some change of character. The 1st, as the stage of increased sensibility; the 2d, the stage of decreased sensibility; the 3d, the paralytic or convulsive stage.

"In the first stage, every stimulus produces a sensation more than proportioned to its common effects. There is great aversion to light, and to sounds; there is watching, sickness, pain, a quick pulse. In the second stage, the child is not easily roused, his pupil is dilated, his pulse slow, he is lethargic, with often an obstinately costive belly. In the third stage, which perhaps may be considered as a continuation of the second, there is squinting, rolling of the head, raving, stupor, convulsions, with a rapid thready pulse."

Amongst the causes of hydrocephalus, there is none perhaps of greater moment than that which depends upon a deranged state of the abdominal viscera, upon a morbid state especially of the liver and alimentary canal. The subject is accordingly investigated with great minuteness, and many facts and observations are brought forward in proof of the intimate connexion between these disorders of the *primæ viæ* and this dangerous

disease of the head; such are the well known association between the actions of these organs; the great disorder observed in the chylopoietic viscera, often for weeks before the appearance of characteristic hydrocephalic symptoms; the removal of well marked symptoms of hydrocephalus by the use of purgatives, and other remedies directed to the bowels; the tenderness of the region of the liver, remarked by the author in children under hydrocephalus; the remains of great inflammatory action in this viscus, observed by him after death; and the very peculiar stools which belong to the disease, indicating a changed state of the biliary secretion.

The appearances on dissection after death from hydrocephalus, are thus concisely stated;

"We generally find within the cranium the veins, particularly those of the membranes on the surface of the brain and lining of the ventricles, gorged with dark blood; sometimes considerable adhesions between the thickening of the membranes, and minute and florid vessels upon the pia mater. The ventricles we find to contain from two to six ounces of limpid serum; also fluid in a small quantity under the tunica arachnoides, both above and at the base of the brain. The substance of the brain is generally soft and blanched, fimbriated, and particularly soft where it forms the ventricles. The substance of the fornix is often like a soft curd. In the abdomen, I have found the intestines inflamed and constricted from spasm, and the surface of the liver of a bright red colour, abounding in minute vessels; and sometimes extensively adhering to the peritoneum. In several dissections, I have found the surface of the liver studded with small white tubercles, not larger than a grain of mustard. The glands of the mesentery are often diseased, as is evinced by their enlargement, and the caseous depositions which we find in the substance of the glands."

In investigating the pathology of hydrocephalus, the opinions of Whyte, Quin, and Rush, are stated and canvassed with much judgment and candour.

The effusion of water, whence the disease has derived its name, is evidently the effect of the disease, and not its cause. In some cases no effusion has been detected; and it is probable



that in every case the effusion does not take place, but in the latter stages of the disease.

The action which takes place in hydrocephalus, is shown to be very different from the inflammation of phrenitis; the opinion of Dr. Quin seems to approach nearer the truth; but it is observed by Dr. Cheyne, that there is some change prior to the morbid accumulation of blood, "there must be an active before there is a passive state." He is inclined to think that the morbid action in the brain is specific; that excitement first takes place, then increased vascular action, congestion, and effusion.

"That in this disease there is produced a venous congestion, in addition to, and probably arising from the increased arterial action: that the effusion of serous fluid arises from this venous congestion: that this effusion has a tendency to counteract the baleful effects of the increased action, and to retard the fatal termination of the disease; of course, that the effusion into the ventricles is not the cause of the violent symptoms; and that the increased arterial action, though perhaps varied, does not cease when the congestion and effusion have taken place."

The stage of torpor he supposes to commence with the congestion of the venous system. Effusion is not considered as the cause of death, because large effusions of serous fluid have long subsisted with the continuance of life, as in chronic hydrocephalus.

"It is the morbid action of the bloodvessels wasting the brain, and unfitting it for its functions, which destroys the child; and I think it no rash opinion that death would occur earlier in the disease, did not the fluid exude, and thus continue to the brain the necessary degree of support from within, which would have been lost by the wasting of the organ."

This, however, we are disposed to consider merely as a bold and ingenious hypothesis. The author has at least shown, that the presence of water in the ventricles is not necessarily fatal to life; though in some of the instances it should be remarked, that the pressure from the fluid may have been obviated by the separation of the bones, and enlargement of the head, as in chronic hydrocephalus; and in other instances it is impossible to say how long the water had been effused before the cessation

of life, or if in some it had not been the immediate cause of death. The stage of excitement and increased sensibility, has been attributed by Dr. Cheyne to the increased vascular action; the stage of torpor to the congestion in the veins; and is it not highly probable, that the paralytic and convulsive stage, which ushers in the death of the patient, marks the effusion into the ventricles?

Having recapitulated the pathological facts unconnected with hypothesis, and observed that we are at present ignorant of any way of subduing hydrocephalus, without the substitution of another constitutional irritation, Dr. Cheyne proceeds to the treatment of the disease. The indications of cure laid down by him are,

—"To remove from the constitution every irritation which either may have given rise to, or may have assisted, by morbid sympathy, in prolonging the diseased action in the brain;—to subdue the hydrocephalic action, 1st, by diminishing the increased activity of the circulation within the head; 2dly, by substituting a new action;—to alleviate pain and sickness, if ever urgent; to accept every assistance offered by counter-irritation; to support and renew the strength; to support it more particularly under any critical discharge."

The connexion which is shown to subsist between irritation in the alimentary canal, and the hydrocephalic excitement, claims, according to this view, a very early attention. Strong cathartic medicines are often required, and in most cases the very best effects may be expected from a course of purgatives.

The practice of Dr. Hamilton, and the great success which has attended the use of purgatives, are already known to the public. The experience of our author confirms the utility of this treatment. His plan is, when called early, to recommend—"the exhibition of the largest dose which can be safely prescribed, of some powerfully cathartic medicine, two, three, or four times a-day; and this continued for several days, or until natural stools are procured. The advantage of keeping the intestinal canal under the continued influence of a stimulus, I have, in various instances, found to be so great, that I am induced to repeat the declaration of my belief, that the happiest result may be expected from this measure."



The increased activity of the circulation within the head, is to be diminished by local or general bloodletting, according to the circumstances of the case; and the abstraction of blood, it is remarked, is useful on another account, to render the constitution susceptible of the powers of mercury.

From blisters he has frequently observed a remarkable mitigation of the symptoms. However, so soon as the existence of the disease becomes probable, no more time should be lost than is necessary for subduing the disorder in the bowels; we must then commence the mercurial course, which, it must be allowed, has cured hydrocephalus even when far advanced. The object of the mercurial course, is to substitute a new action or counter-irritation, by which the hydrocephalic action shall be suspended or subdued: examples of which have come under our author's observation, and under that of other physicians. Digitalis is a remedy of considerable promise in this disease, from the command which it has over the circulating and absorbent systems. The author's experience of this medicine in hydrocephalus, has not been great; but he has observed it to produce a great, and in two cases he conceives a salutary, influence.

A valuable collection of cases is annexed to this essay. From these cases the results and doctrines of the text have been drawn. They are of themselves important and instructive. "They were invariably drawn up in the bed-chamber of the patient, and present a faithful account of every symptom and circumstance that attended the disease."

*Practical Observations in Surgery, illustrated by Cases. The second Edition, corrected and enlarged; with additional plates.*

BY WILLIAM HEY, F. R. S.

Member of the Royal College of Surgeons in London; Honorary Member of the Royal Medical Society of Edinburgh, and of the Literary and Philosophical Society of Manchester; and Senior Surgeon of the General Infirmary of Leeds. 8vo. pp. 578. Price 13s. London. Cadell and Davies. 1810.

From the London Medical Review for 1810.

THIS is a new and much enlarged edition of one of the most valuable contributions that has ever been made to surgery. So much knowledge without pretension, improved by observation so well conducted and matured, are equally calculated to arrest the public confidence and to maintain it by just decisions.

With exemplary candour, and a degree of zeal exceedingly rare, at least among men of the author's years, Mr. Hey was no sooner apprised of the incorrectness of some terms in his original description of the anatomy of femoral hernia, than he determined to examine for himself the modern illustrations of this important subject. With this view he visited the schools of the metropolis, and with all the ardour of a youth entering on his studies, attended the lectures and examined the preparations of Mr. Astley Cooper. Such an undertaking however, was only accomplished at the imminent risk of his life. In the present edition Mr. Hey has in consequence amplified and illustrated by plates, with perfect accuracy, the anatomy of femoral hernia. In the preface he thus acknowledges and very satisfactorily explains his former misconceptions.

"When I first committed my papers to the press, the public had not been favoured with that elaborate and excellent work of Mr. Astley Cooper on Hernia. I had, indeed, read the treatise on femoral hernia by Don Antonio de Gimbernat; but had not profited by it as I might have done. For not understanding clearly, at my first perusal, his description of the posterior projection of the aponeurosis of the external oblique muscle of the abdomen, I incautiously laid the work aside;



determining, however, to seize such opportunities as might offer of discovering the cause of stricture in femoral hernia.

"On such occasions, I pursued my examination in the following manner. Having laid bare that part of the fascia lata of the thigh, which covers the great femoral vessels, where they descend below Poupart's ligament; I opened the abdomen, and removed the peritoneum, together with that fatty membrane which lies at the entrance of the sheath of those vessels. I then dissected out the lymphatic glands and adipose membrane, which remained in the sheath on the inner side of the femoral vein. Having cleaned these parts, I introduced my finger into the sheath; and, carrying it downwards on the inner side of the vein, till it appeared below what is now called the lunated or falciform process of the fascia lata, I took notice where the stricture upon my finger was the greatest. In doing this I found the anterior edge of the thin projection of the aponeurosis of the external oblique muscle, to coincide or be continued with the falciform edge of the fascia lata. This part I called the femoral ligament. My idea, however, of the anatomy of these parts was not clear when I first adopted that term; and, consequently, my description of them was obscure; though I am not aware that it led to any practical error."

Besides the great additions to the chapter on the strangulated hernia, and other considerable alterations in various parts of the work, the author has annexed to this edition three short chapters on the following subjects. On the hydrocele of the spermatic chord—a case of lithotomy in the female—a case of tumor on the nose. An appendix to the volume contains a case from Mr. A. Cooper of strangulated femoral hernia with moruified intestine.

We shall offer a very few remarks on some points of the work. The convex-edged saw which among us has pretty generally obtained the appellation of Hey's saw, deserves in our opinion, all the encomium which has been passed upon it. The trephine is an instrument which is often quite inapplicable, and has only kept its ground for want of a better; this is now supplied.

In his chapter on the cataract Mr. H. exposes the inaccuracy of the idea conveyed by some writers when they speak of

depressing the crystalline in the posterior chamber. This is however merely a difference in the application of the term posterior chamber, between writers who use it; their anatomy, it cannot be questioned, is the same; their nomenclature here as in innumerable instances at variance. Winslow's description is for obvious reasons the correct one. There exists no other chamber posterior to the iris than that occupied by the aqueous humor. The operation of depressing the cataract is that which Mr. H. is in the habit of performing. His instrument differs from that generally used in its length, being only seven-eighths of an inch; in its shape, being elliptical instead of pointed at its extremity, and from the part where it ceases to be flat, increasing gradually in diameter towards the handle. This construction he considers to be attended with decided advantages; but they appear to us to be founded upon a comparison with very ill constructed needles of the spear shape. We allow that a short needle is the best, but one needle may be as short as another; that increasing in diameter towards the handle is advantageous, but such a construction is common to all the needles which we have seen used. "This needle has no projecting edges, nor any projecting point," says Mr. H. These we reckon among its disadvantages, for if it had projecting edges, the operator has no business to wound the ciliary processes or iris, and if he wounds them with one instrument he will with another; but angular edges will do more execution than parallel ones, and if it had a projecting point it would enter the globe of the eye with as much facility and as little pressure as the spear-shaped needle, which we can say from experience it *does not*.

Repeated efforts are stated to be more successful than those which are long continued, to depress the firm cataract, but the soft cataract Mr. H. finds to be the more frequent. "In this state of the disease, I do nothing more," says our author, "than break down the texture of the cataract, and endeavour to puncture or tear off a portion of the capsule, that the aqueous humor may flow in upon the broken cataract. In doing this it is common to see some fragments of the cataract fall through the pupil into the anterior chamber of the eye. I am always glad to see this take place, as I then know that there is a passage



opened for the admission of the aqueous humor, and that those opaque fragments which have passed through the pupil will soon disappear." Mr. Hey inculcates great caution in the conduct of the operation, and very strongly deprecates an opposite mode of procedure. The repetition of the operation he considers to be no discredit to it, nor any discouragement to the patient if he be properly made aware of its necessity. He has succeeded in destroying the strict adhesion of the iris to the capsule, by repeating the operation in some instances even eleven or twelve times. In the course of this chapter many observations occur which are clearly the result of extensive practical experience. We expect ere long to have occasion to renew this subject in detail, and shall therefore defer a critical inquiry into the merits of the several operations practised for the cataract.

The anatomy of inguinal and femoral hernia is described with much conciseness and perspicuity. Mr. H. comments usefully on the division of the stricture in the latter disease, and considers that some obscurity has been occasioned by an ambiguous use of terms. *Upwards*, for example, in the supine posture, is *horizontal* in relation to the erect posture. This is an instance in point, of the improper use of common terms relating to position and aspect. "Supposing," says Mr. H. "that the obturator artery passed round the neck of the sac, but at such a distance from the ring that the button of the bubonocoele knife could pass freely between it (the artery) and the stricture; it is evident that in making the incision *upwards*, as it is called, the knife would have made no greater approach towards the artery at the end than at the beginning of the incision." On the treatment of gangrene of the intestine, Mr. H. refers to other writers for information. "I have seen several such cases, but the termination of them in general was fatal." After relating two cases of recovery, he conjectures that many recoveries may have been owing to the head of the colon being the sphacelated portion.\* This suggestion may be confirmed by the circumstances of some few cases, but it is quite inadequate as a general explanation.

\* Vid. Med. Obser. and Inq. Vol. III. Art. 8.

We are presented with some instructive histories of the several modes of treating adhering and mortified omentum. It is by no means yet decided whether reduction, excision, or leaving the omentum in the wound when much altered by disease, is the preferable practice. Reducing an adhering portion of epiplocele and retrenching it have both proved fatal in Mr. H.'s practice. To the ligature he feels all the aversion expressed by the late Mr. Pott, having been a witness of its destructive effect in the very case upon which Mr. Pott's objections to it were founded. On the other hand, a portion of omentum of several ounces weight, he has left in the wound after the removal of the stricture, and after a third of it had sloughed, removed the remaining two thirds to the level of the parietes by the application of a ligature. By frequently renewing this application he succeeded in removing a portion weighing five ounces and five drams avoirdupois, in seventeen days, and in six weeks the wound was nearly cicatrized.\*

The result of Mr. Hey's practice has convinced him that large wounds of the omentum are dangerous if the bleeding vessels are not tied. "I shall never again cut off any large portion of omentum without applying a ligature to every bleeding vessel before I permit the remainder of the omentum to retire into the abdomen." But he confesses that with this precaution he has little apprehension of ill consequences, and there are cases in which its excision must be beneficial. Mr. Pott who, though in the main a very good surgeon, was a very great dogmatist, has spoken with a degree of temerity on this subject, which nothing but the candor of a weak man or the infatuation of a prejudiced one will excuse.

The knee joint is subject to some internal derangement from slight accidents, as twists, sprains, and blows. This is probably a partial dislocation of the convex articular surfaces of the femur, from the semilunar cartilages of the tibia. Mr. Hey, who first noticed the case, has invariably given instant relief by a complete flexion of the leg while the patient was in the sitting posture, as on the edge of a table. A well adapted laced knee-

\* Mr. Hey found the practice of leaving gangrened omentum in the wound to answer well in three cases in which he has tried it. In two the diseased part was cast off on the 7th day; in the third on the 11th.



cap Mr. H. has found of very essential service in the cases of loose cartilaginous substances in the joints. This practice has not uniformly succeeded in removing the lameness and pain, or preventing the return of the disease. We have seen these substances which are as much bony as cartilaginous, removed with considerable difficulty, and very severe symptoms follow the operation.\* Very lately however, a case has occurred in which the operation was done in such a way as to prevent symptoms of irritation. This consisted in making the orifices of the skin and of the ligament at a considerable distance apart, by previously stretching the integument. A valvular covering to the internal incision was thus afforded by the skin resuming its situation, so that no more irritation succeeded than if the wound had not extended beyond it.

The successful termination of so many cases of wounds and compound luxations of the joints, convinces us, with the highest respect for the skill of the Leeds Infirmary, that its surgeons have constitutions to work upon which give them a high vantage ground over those of the metropolitan hospitals. The chapters on retention of urine, and on amputation deserve very attentive perusal.

Several distressing cases of *procidencia ani* the author has completely relieved by removing with the knife the flaps of skin and tubercles that lie on the side of the anus, inducing by this means adhesion of the cellular substance to the replaced gut.

We feel somewhat disposed to doubt if Mr. H. has properly named all the cases included under the head "Cancer of the Penis." In eight out of ten, there was a congenital phymosis. Now we believe that this fact has a relation to the origin of the disease, and therefore renders it vastly improbable that it was cancer. We have little doubt that it was so in case 4, but we are rather surprised that it did not occur to Mr. H. that the confinement of the matter secreted by the *glandulae odoriferae*, the irritation of surfaces unnaturally opposed, in the venereal act, &c. might account for a fretful ulceration and an unhealthy discharge which in its appearance, not in its nature,

\* See the Medical and Surgical intelligence of the present month. [July.]

resembled cancer. It strikes us upon the aspect of the cases, that he was led to consider the disease carcinomatous upon too slight evidence: that he exercised less than his usual deliberation of decision. We may be mistaken in the fact: we judge only from the document before us. We know that the texture of the prepuce and skin of the glans penis is subject to excrescences which fungate and ulcerate, and give the surgeon much trouble. But it is remarkable (if the disease was cancer) that it should only in one of eight cases affect the neighbouring glands; that not one of the eight patients should have any return of the disease; that the ulcerated prepuce and frænum having been extirpated, the wound should cicatrize in a fortnight, and the penis remain sound (case 6); that a mere division of the prepuce should cure the disease, as in all probability it did, since Mr. H. has heard nothing of his patient since the cure, which was several years ago. (Case 7.)

In a case precisely corresponding to Mr. H.'s description, which some little time ago brought a country gentleman to London, the part was condemned to be amputated by a surgeon of the first eminence, which so alarmed the gentleman, yet vigorous in years and constitution, that he consulted another surgeon of equal eminence, who laid bare the penis to the extent of the disease, and cured him by local applications.

We shall conclude these remarks by the extract of Mr. Hey's last case (10) in his own words, which will prove, we think, that the question is not merely of importance in pathology, but in practice, whether this disease of the prepuce, for there it commences, is or is not cancerous. If it is decided in the negative, the following practice is unnecessarily severe: whatever it be, one most important practical inference which we are surprised that our author should have omitted to make, is furnished by these cases; *a congenital phymosis ought never to be suffered to exist beyond the age of puberty.* We can say from a recent performance of the operation merely to remove the inconvenience, that it is attended with perfect relief if the division is of sufficient extent, and is followed by very trifling deformity.

"Jonas Royds, aged 76 years, who had a congenital *phymosis*, about nine months before his admission into the Leeds



Infirmity, (March 16, 1810) perceived an enlargement of the extremity of the penis, especially on one side. The diseased part felt hard, and was prominent. A bloody serum issued frequently from the prepuce, and the discharge of his urine gave him pain. Nothing morbid appeared in the integuments; nor was any ulceration perceptible in the prepuce. In other respects the patient was healthy.

"20th, I amputated the diseased part; and, upon examination, found one half of the prepuce ulcerated internally, and covered with a cancerous excrescence, which extended along the corona glandis, from the *frænum* to the *dorsum penis*. The rest of the glans was in a sound state; and the opposite side of the prepuce was but slightly affected."

Mr. Hey has ingeniously contrived to prevent the involuntary discharge of urine after the operation of lithotomy in the female, by filling the vagina with a plug of rolled linen. It answered well in a case which he relates; but we have good hope that the operation may be rendered altogether unnecessary, from the example of perfect success which we have given in our medical and surgical intelligence of the present number. The revival of the practice of dilatation of the *meatus urinaris* for the removal of foreign bodies or calculi from the bladder, is due to Mr. Thomas; and from this gentleman's paper in the Transactions of the Medical and Chirurgical Society, Mr. Cooper, ever anxious for professional improvement, probably borrowed the idea.

It only remains that we should notice the case contributed by the last mentioned gentleman to the work before us: it is one of infinite value. The circumstances are shortly these: "The patient, a woman three months advanced in pregnancy, was reduced to a state of extreme lowness by a strangulation of four days' continuance; the mortified gut was opened so as to admit of a fecal evacuation at the groin; on the 8th day she passed flatus, on the 8th fæces by the rectum, and continued to do so occasionally and in part for the space of a month, when the fecal discharge took its natural course uniformly, and the wound in the inguen closed. This happened in the month of *May*, 1808. She was delivered in the October following of a full grown dead child, and some time after miscarried; from

that time she has continued well in every respect, and only wears the truss when exposed to unusual fatigue." The following is Mr. Cooper's conclusion.

"From the foregoing history it appears that the proper treatment of a mortified intestine in strangulated hernia, consists in the two following circumstances:

"1st. In dividing the stricture so as entirely to remove the cause of strangulation; and,

"2dly. In making an opening into the intestine so as to give a free outlet for the discharge of the accumulated fæces in the intestine within the abdomen. If the stricture only is divided, the constipation, hiccough, and vomiting, continue; but if the intestine is opened, the patient in a few hours becomes relieved of those symptoms.

"This case will serve as an answer to a query in the London Medical Review for April 1808. 'It remains for the candid observation of those who have the opportunity to determine, Whether in cases where the patient survives the operation, the removal of gangrenous intestine by the scissors is, or is not, an objectionable practice? *and whether it is giving a fairer chance of recovery to second the efforts of the constitution during the process of separation, while we facilitate by a free opening the evacuation of the alimentary matters?*'"

We shall for the present forbear to comment on this interesting case further than to congratulate ourselves on having suggested the practice to which, as we believe, this woman owes her life.

Nothing indeed can be more opposite to this practice than that which Mr. Cooper has laid down in his work on hernia, viz. the removal of the mortified portion of intestine, and the connexion of the extremities by sutures; and we regard this as an honourable proof that his interest in the advancement of the profession overpowers those little feelings and political calculations, which have too often induced in men of eminence a pertinacious adherence to their published opinions.

The case which occurred in June, is an incontrovertible instance of the practical expediency of the proposal which we suggested in May preceding. (L. M. R. vol. I. p. 131, 132.) This fact Mr. Cooper has handsomely acknowledged, by pub-



lishing it in the form of a reply. If the obvious conclusion should be borne out by future results, it will be a subject of gratification to us, to reflect that the London Medical Review has not lived in vain.

*Non ex quovis ligno Mercurius fit.*

We cannot close this volume without declaring that in the course of our critical labours, we have never closed one with a stronger conviction of its value, or a higher respect for its author. We recommend it without hesitation or reserve to three classes of persons; first, to writers on practical subjects as a model of useful composition both in substance and in style; secondly, to established surgeons, as a book of reference for many valuable facts in practice; thirdly, to students, as a book which the smallest professional library should contain, and the largest cannot be complete without.

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*The Principles of Midwifery; including the Diseases of Women and Children.\* By John Burns, Lecturer on Midwifery, and Member of the Faculty of Physicians and Surgeons, Glasgow, London, 1809. 8vo. pp. 519.*

From the Edinburgh Medical and Surgical Journal for 1810.

WE already possess some excellent treatises on the principles and practice of midwifery; and, as this is a department in which no very important discoveries or improvements have very lately been made, we should have thought it at this time no very easy matter to usher into public, with any fair prospect of success, a new publication on the subject. All, however, that could be expected, in the present state of the science, to give a new interest to the subject, Mr. Burns, equally experienced as a teacher and practitioner, has, by a judicious arrangement, by a faithful exposition of facts and observations, and by a methodical induction of the principles and practice

\* An edition of this very valuable work, with notes, has been recently published by Dr. Chapman, Lecturer on Midwifery, Philadelphia.

of the art, from these fundamental truths, contrived to accomplish in the work now before us. It is divided into four books; the first, comprehending nineteen chapters, treats of the structure, functions, and diseases of the pelvis and uterine system, in the unimpregnated state, and during gestation; the second, of parturition; the third, of the puerperal state; and the fourth, of the management and diseases of children. So that every subject, directly or indirectly connected with the practice of the accoucheur, is here brought into one connected view. This, indeed, is the great advantage which Mr. Burns's work must be acknowledged to possess over all others, as a system or class-book. But what we are most disposed to recommend in this volume, is the pathological department, and the descriptions and treatment of the diseases of puerperal women and of children. A more copious, scientific, and judicious account of these diseases, is, perhaps, no where to be met with.

To enumerate some of these, in the order in which they are treated, will convey to our readers some notion of the value of this part of our author's labours.

1st. Diseases of pregnant women:

Febrile state; vomiting; heart-burn; fastidious taste; spasm of stomach and duodenum; costiveness; diarrhœa; piles; affections of the bladder; jaundice; coloured spots; palpitation; syncope; dyspnœa and cough; hæmoptysis and hæmatemesis; headach and convulsions; toothach; salivation; mastodynia; œdema; ascites; redundancy of liquor amnii; watery discharge; varicose veins; muscular pains; spasm of ureter; cramp; distention of the abdomen; hernia; despondency; retroversion of uterus; antiversion; rupture of uterus; abortion, and treatment of pregnant women; uterine hemorrhage; false pains.

2d. Of the Puerperal State:

Treatment after delivery; uterine hemorrhage; inversion of the uterus; after pains; suppression of the lochia; retention of part of the placenta; strangury; spasmodic and nervous diseases; milk fever; miliary fever; inflammation of the uterus; peritoneal inflammation; puerperal fever; swelled leg; paralysis; mania, &c.

3d. Diseases of Children:

Congenite; surgical diseases; diseases of dentition; cutane-



ous diseases, viz. strophulus, lichen, intertrigo, crusta lactea; anomalous eruptions; pompholyx; pemphigus; miliaria; prurigo; itch; herpes; itchthyosis; psoriasis; impetigo; pityriasis; porrigo; scabs; boils and pustules; petechiæ; erysipelas and erythema; excoriation; ulceration of gums; erosion of cheek; aphthæ; syphilis; skin-bound; small-pox; cow-pox; chicken-pox; urticaria; measles, and roseola.

Hydrocephalus; convulsions; croup; whooping-cough; catarrh; inflammation of pleura and stomach; vomiting; diarrhœa; costiveness; chorea; marasmus; tabes mesenterica; worms; jaundice; diseased liver; fevers.

A systematic work like this, embracing so great a variety of description, does not admit of any more extended analysis, or criticism; we shall, however, to enable our readers to form some judgment of the manner and style of Mr. Burns, add a very few extracts.

“Chlorosis succeeding to abortion, laborious parturition, or fever, is often attended with symptoms resembling phthisis pulmonalis. In many instances the pulse continues long frequent; there is nocturnal perspiration, considerable emaciation, with cough and pains about the chest, and yet the person is not phthisical, she suffers chiefly from debility; but if great attention be not paid to improve the health, the case may end in consumption; and hence many consumptive women date the commencement of their complaints from an abortion, or from the birth of a child, succeeded by an hemorrhage. In chlorosis, the symptoms are induced, not by previous pulmonic affections, but by some other evident cause of weakness; the pulse, although frequent, is not liable to the same regular exacerbation as in hectic; a full inspiration gives no pain, and little excitement to cough; the person can lie with equal ease on either side; the cough is not increased by motion, nor by going to bed, but is often worst in the morning, and is accompanied with a trifling expectoration of phlegm. It is not short like that excited by tubercles, but comes in fits, and is sometimes convulsive; whilst palpitation, and many hysterical affections, with a timid and desponding mind, accompany the symptoms. The bowels are generally costive, and the person does not digest well.

"In chlorosis attended with symptoms resembling phthisis, it is of considerable utility to administer, occasionally, a gentle emetic, and at the same time, the bowels must be kept open. Myrrh combined with the oxyd of zinc, is, I think, of approved efficacy; and the ammonia given in the form of an emulsion, with oil, very often is effectual in removing the cough. A removal to the country, and the use of moderate exercise on horseback, will contribute greatly to the recovery. The diet ought to be light but nourishing. In many cases, milk agrees well with the patient, but it is not necessary to restrict her from animal food. Pain in the side may be removed by the application of a warm plaster; and if the cough be troublesome, the squill may be used as an expectorant, and an opiate should be given at bed-time. If the skin be permanently hot, or irregularly hot and cold, without weakening perspiration, the tepid bath is of service, or small doses of saline julep may be given. Should the person be of a phthisical habit, and the symptoms increase or continue obstinate, it will be proper to remove her to a mild climate, or the southern part of the island. Emmenagogues are either useless or detrimental." p. 100-1.

After having described the operation of embryulcio, the cases requiring it, and the necessity of breaking down the cranium and separating the bones, in cases of extreme deformity, Mr. Burns continues:

—"We have then only the face and the basis of the skull, and if the pelvis will allow these remains to pass, then the crotchet can be used. I have carefully measured these parts, placed in different ways, and entirely agree with Dr. Hull, a practitioner of great judgment and ability, that the smallest diameter offered, is that which extends from the root of the nose to the chin. For in my experiments, after the frontal bones were completely removed, this did not in general exceed an inch and a half. It is, therefore, of great advantage to convert the case into a face presentation, with the root of the nose directed to the pubes. The size of the crotchet, which ought to be passed over the root of the nose, and fixed on the sphenoid bone, must, however, be added to this measurement. I never have yet been so unfortunate as to meet with what may



be considered as the smallest pelvis admitting of delivery, *per vias naturales*; but I would conclude, that wherever the pelvis, with the soft parts, measures an inch and three quarters, or if the head be unusually small, an inch and a half, the crotchet may be employed, provided the lateral diameter of the aperture in the pelvis be three inches, or within a fraction of that, perhaps two inches and three quarters, if the head be small or very soft; and the operation will be easy, as we extend the diameter of the pelvis, beyond what may be considered as the minimum." p. 264-5.

The cutaneous diseases of children, so numerous, and often so difficult to discriminate, are very fully described by Mr. Burns, under no fewer than thirty-seven different sections. We select the following excellent account of a very dreadful disease affecting the face and mouth:

—"Another corroding disease begins in the cheek itself, or the lip. It commences with some degree of swelling, which is hard, and firm, and shining. It generally begins on the cheek, which becomes larger than the other; and the upper lip becomes rigid, swollen, and glossy. On some part of the tumefied skin, generally on the cheek, we observe presently a livid spot, which ulcerates and spreads, both laterally and downwards. Being generally situated near the mouth, it soon reaches the gums, and even the tongue partakes of this disease, which is of horrible aspect. We often find a great part of the upper or under lip destroyed, perhaps only a flap or portion of the *prolabium* left, all the rest being eaten away. The gums are foul, the teeth loose, the tongue thickened, partly destroyed, and lying so close on other diseased parts, that we cannot say what is tongue or what gums, except by the child moving the tongue, and the mouth itself is filled with saliva. The ulcer is foul, shows no granulations, but appears covered with a rough irregular coat of brown lymph. The surrounding parts are somewhat swelled; near the ulcer they are hard and red; farther out on the cheek they are paler, and have more of an *œdematous* look. These local appearances are accompanied with emaciation and fever, and the child is either restless, or lies moaning in a drowsy state. This disease often proves fatal, sometimes indeed the parts cicatrize, or the patient reco-

vers after an exfoliation of part of the jaw bone. The ulcer is best managed with stimulants, such as diluted muriatic acid, solution of nitrate of silver, camphorated spirits of wine, tincture of opium, &c.; but sometimes it is necessary to give these up for a carrot, or fermenting poultice. The bowels are to be kept open, the strength supported by milk, soups, and wine, and acids, with ripe fruit given liberally. Before ulceration takes place, the best application is camphorated spirit of wine, or we employ friction, with camphorated liniment. A course of gentle laxatives is useful." p. 375-6.

In closing our brief account of this very useful book, we must not overlook the many instructive notes and references to cases and observations, which Mr. Burns has appended to his work, by way of illustrating his text, and enabling his readers to prosecute the study of the subjects to which these refer.



## ORIGINAL PAPERS.

*Experiments on the Sulphate of Barytes, found at Perkiomen.*

BY JAMES CUTBUSH,

Lecturer on Chemistry, Philadelphia.

SOME time since, I made a few experiments on the sulphate of barytes found at Perkiomen, which accompanies several of the ores, in order to ascertain if the component parts corresponded to the analysis of the baroselenite examined by Dr. Withering.

I pulverized a sufficient quantity of the sulphate, which appeared to be crystallized in a tabular form,\* and boiled one part of the powder with four of carbonate of potash, in twelve of water.

The water was renewed as it evaporated, and the mixture was agitated frequently.

The whole was now filtered, to separate the insoluble powder, which was repeatedly washed, and the fluid was preserved.

The insoluble portion was transferred into diluted nitric acid; an effervescence immediately ensued, and a solution was effected. The nitric solution was now evaporated, and the saline matter, thus obtained, was exposed in the manner mentioned by Accum, vol. ii. p. 244, to a white heat for some hours. The quantity of the pure barytic earth now obtained,

\* The primitive figure of crystallized sulphate of barytes, is a four-sided prism at right angles with its base; which is a parallelogram with angles of  $101^{\circ} 32'$ , and  $78^{\circ} 28'$ . These angles almost correspond with those of the primitive crystal of carbonate of lime.

Some of the mineralogical gentlemen of this city have said, that the sulphate of barytes found at Perkiomen, was not crystallized, strictly speaking.

The most common form of the crystal is tabular, with beveled edges.

One fact, however, is certain, that whether the sulphate of barytes found at this place, is amorphous or crystallized, it approaches the state so nearly, that it may be referred to the tabular figure.

was correctly noted. The filtered liquor, which contained the sulphuric acid of the sulphate of barytes, was then saturated with nitric acid, (as it contained a portion of free alkali) and nitrate of barytes was added, until no further precipitate was formed. This precipitate was carefully collected, washed, dried, and weighed. Every one hundred grains indicated, according to the established datum, twenty-three and a half of sulphuric acid.

The small residue left after the action of nitric acid, appeared to be silicious.

The nitric solution contained a portion of iron; for, on adding the tincture of galls to a small quantity, a slight black colour appeared.

By determining the quantity of acid and barytes, obtained from a given quantity of the sulphate, in the manner here stated, the proportion of barytes and sulphuric acid stands thus, as nearly as possible:

Barytes	-	-	-	65
Sulphuric acid	-	-	-	28
Iron, silica, loss	-	-	-	7
				<hr/>
				100
				<hr/>

The theory of this analysis is obvious: the sulphate of barytes is first decomposed by carbonate of potash; the sulphuric acid unites with the potash, which remains in solution, whilst the carbonic acid combines with the barytes, and forms the insoluble powder.

The carbonate of barytes was then treated with nitric acid; carbonic acid was disengaged, and the nitrate of barytes was formed. This was then exposed to an intense heat, to expel the acid, and the barytes remained in a pure state.

The fluid which passed the filter, and which contained the sulphuric acid of the sulphate, was then mixed with a portion of nitric acid, to saturate the uncombined alkali, and precipitated by nitrate of barytes.

The whole of the sulphuric acid was then thrown down, and the precipitate, when collected, &c. gave the proportion of the



acid. By reducing the quantities of the relative ingredients, as given before, the proportion of each per centum was ascertained.

The examination of a specimen of sulphate of barytes, by Dr. Withering, gave 67.2 of barytes, and 32.8 of sulphuric acid.

The Bologna stone, which possesses the phosphorescent property mentioned by chemical writers, although it contains sulphate of barytes, is different in its composition from the *heavy spar* of Perkiomen. In the *Ann. de Chem.* (1788), the analysis of this stone is given. It is met with in a gray argillaceous marl in Monte Palerno, near Bologna. The component parts are stated as follows:

Sulphate of barytes	-	-	62
Silex	-	-	16
Alumine	-	-	14.15
Sulphate of lime	-	-	6
<hr/>			
			98.15
<hr/>			

Klaproth (*Analytical Essays*, vol. ii. page 227) examined a fibrous variety of sulphate of barytes, which was supposed to have been calamine, obtained from New Leiningen, in the Palatinate. He decomposed 300 grains by 600 grains of carbonate of potash, and the carbonate of barytes, thus procured, he dissolved in muriatic acid.

After obtaining the muriate of barytes in a crystallized state, he redissolved it, and added the fluid procured by filtration. As this contained the sulphuric acid of the sulphate, in union with the potash, it precipitated the barytes from the muriatic acid, so that by this process he reproduced the sulphate of barytes. Its weight, after washing and drying, amounted to 297 grains.

From the analysis of the ponderous spar, or sulphate of barytes, presented to us by different analysts, it would appear, that this mineral varies considerably in the proportion of its constituent parts. It is stated by some to contain generally 84 of barytes, and 16 of sulphuric acid, in the hundred.

I have only to remark, however, that so far as barytes is useful either in chemical researches, or in medicine, the sulphate may be decomposed by carbonate of potash, or carbonate of soda, or by decomposing its acid by means of charcoal, and thus afford a means of converting one of the productions of our country to these purposes. The subsequent solution in nitric or muriatic acid, would produce the nitrate or muriate of barytes.

To obtain the muriate from the sulphate of barytes, Mr. Klaproth boiled 16 ounces with 32 ounces of carbonate of potash, and 5lbs. of water, for an hour, in a tin kettle, constantly agitating the mixture, and renewing the water as it evaporated. After filtering it, and washing the carbonate of barytes, to free it of sulphate of potash, and any portion of uncombined alkali, he dissolved the carbonate in pure muriatic acid. The undecomposed sulphate, if any should remain, may be treated again in the same manner.

Van Mons, Westrumb, and Goettling, have severally given us some observations relative to the decomposition of sulphate of barytes, in order to prepare the muriate; but, as the subject would occupy our attention too far in this place, we will only add, that the conversion of the sulphate into the muriate of barytes, for chemical purposes, is a desideratum in our country, in which the native carbonate (barolite) has not been discovered, to my knowledge. Any of the preparations of barytes, may, by thus freeing the earth of sulphuric acid, be very readily formed.

*August 15, 1810.*



*Some Experiments on the Coagulation of the Blood when out of the Body.*

BY THOMAS T. HEWSON.

THE blood is fluid while circulating in the vessels of the living animal; but when taken from the body it soon forms a soft tremulous mass. This conversion of the blood from a fluid to a solid state, is termed its coagulation; and must have been familiar to mankind from the earliest ages. The slaughter of animals, the bleedings from wounds inflicted by accident or design, could not fail to establish the fact. The circumstances, however, which favour or prevent the coagulation of the blood seem not to have been, till very lately, the subject of experimental inquiry.

As the temperature of the human body generally surpasses that of the circumambient air, it has been inferred, that the coagulation of the blood, like the congelation of water, depended on the mere abstraction of heat. The analogy of size, and animal jelly, which are fluid in the temperature of our bodies, and become solid when exposed to the medium temperature of the atmosphere, seems to give strength to this opinion. There is, however, this essential difference, that water and jelly assume the solid, or fluid state, according to the degree of heat which is applied; but when once blood has coagulated, it is incapable of being rendered fluid by any additional heat. And farther, it has been very satisfactorily proved by the experiments of Mr. Hewson and Mr. Hunter, that the blood may be actually frozen and converted into ice before it has time to coagulate; and yet on being thawed it coagulates like blood that had never been frozen. Mr. Hewson has likewise shown that the blood coagulates as readily when placed in a heat of  $100^{\circ}$ , which is equal to the natural temperature of our bodies, as in a heat of  $67^{\circ}$ , and a heat of  $120^{\circ}$  produces a more immediate coagulation.

The continual agitation which the blood undergoes during its circulation through the body, has been considered by many as the principal means of maintaining its fluidity; and it has

been urged, that *rest* alone was sufficient to occasion the coagulation of the blood. In support of this opinion, its advocates have adduced the familiar experiment of agitating the blood, when received in a bason, by means of a bunch of rods or small sticks; in which case it is asserted the coagulation will be prevented. If we scrutinize into the circumstances attendant on this experiment, there will be no difficulty in detecting the nature of the deception, and in showing the complete futility of the argument. In fact, the coagulation is complete, and the sticks will be found loaded with solid matter, that part of the blood which is alone capable of spontaneous coagulation, and freed from it, the other parts remain in their natural fluid state.

With the view of acquiring more precise ideas respecting the comparative influence of agitation or rest in the coagulation of the blood, I made the following experiments. About six ounces of blood were received, from a vein in the arm, in a white earthen bowl; the time required for collecting this quantity was three minutes. While the blood was yet flowing from the vein, it was kept constantly agitated with a small bunch of slender sticks. In five minutes the coagulation had evidently commenced, and at the end of five minutes more it appeared to be complete. The whole time occupied in this experiment was thirteen minutes. After the removal of the small clot which adhered to the sticks, the remaining blood was set apart for further examination; and on the next day it still continued fluid, a proof that the whole of the coagulating principle had been removed. In farther confirmation it may be mentioned, that the coagulum, when washed and dried, weighed thirteen grains, equal to the largest proportion of fibrine, I have ever been able to detect in the blood.

Immediately as the first bowl was removed a similar one was employed to receive a like quantity of blood; and here the time occupied in collecting it was likewise three minutes. It was not till the end of ten minutes after it was set by to rest, that there was any appearance of coagulation, and five minutes more were required before it was complete, making in all a period of eighteen minutes.



Hence it appears reasonable to conclude that motion so far from preventing or retarding the coagulation of the blood, actually tends to quicken this process; and consequently that the coagulation of the blood out of the body, cannot be considered as occasioned by rest alone.

It is a common observation that the blood coagulates more readily in broad shallow vessels than in those which are narrow and deep: this has been ascribed to the action of the air. With a view of determining this point Mr. Hewson instituted a number of experiments, the history of which he communicated to the Royal Society in the spring of 1771; and which he afterwards presented to the public in a separate volume. From comparing these experiments\* he ventures on the following conclusion: that "the air is a strong coagulant of the blood, and that to this, its coagulation, when taken from the veins, is chiefly owing; and not to cold, nor to rest." Mr. Hunter, on the contrary, after referring to the experiments and observations of Mr. Hewson, asserts,† "that the air has really no more effect than any other extraneous body, in contact with the blood, that is capable of making an impression upon it; for the blood coagulates more readily in a vacuum than in the open air." This has been assumed on the authority of Mr. Hunter, and adopted by Murray in that part of his *Elements of Chemistry*‡ which contains the history and analysis of the blood. Mr. Allen, formerly a lecturer in Edinburgh, is, I believe, the only one who has publicly expressed any doubts respecting the accuracy of Mr. Hunter's observation. As Mr. Hunter has not described the manner in which this experiment was conducted, we are at a loss to determine whether the vacuum he employed were perfect or not.

It appeared to me that a common injecting syringe might be filled with blood immediately as it flowed from the body, without coming in contact with the air, in which case the agency of the air in the coagulation of the blood would be ascertained.

Having opened the vein in the arm, three ounces of blood were received in a tin vessel. The mouth of a brass syringe

\* Hewson's *Inquiries on the Blood*, p. 21.

† See Hunter on the Blood, Vol. i. page 20, Amer. Edit.

‡ Vol. iv. page 526.

was next applied to the bleeding orifice, a piece of moistened leather fastened to the end of the syringe, and made to embrace the arm, served to exclude the air. The piston was then very gradually raised, and the syringe filled with blood, three ounces, and then corked. About four ounces of blood were taken in an earthen dish. These parcels of blood were placed in a window. At the end of twenty-five minutes the first and last portions were firmly coagulated. Upon removing the cap of the syringe I found about three drams of coagulated matter, having but little admixture of the red globules: the remainder of the blood was fluid, and coagulated on being poured into the dish.

From comparing all that has been stated above, may we not join with Mr. Hewson in concluding that the air is a strong coagulant of the blood, and that to this its coagulation, when taken from the veins, is chiefly owing; and not to cold, nor to rest?



## ORIGINAL REVIEW.

*An Inaugural Dissertation on the Medical Virtues of the White Oxide of Bismuth, &c. &c.*

" — omnes boni medici ad ea remedia semper confugient, (sive modus  
 " quo prosunt intellectus fuerit necne) quæ certa experientia prodesse  
 " comprobaverit." *Greg. Consp. Med. Theoret.*

*By Samuel W. Moore, A. B.—New-York, printed by T. & J. Swords, 1810.*

THIS is an interesting and well digested essay. It is written in a style plain and familiar, and the arrangement of its contents is exceedingly perspicuous.

The object of the author is to call the attention of practitioners to a medicine which really seems to us to have been too much neglected. In the execution of this design, he has diligently collected, and brought together within a very narrow compass, much important information concerning its properties, which was previously scattered through a variety of periodical works, and other sources, of no easy access to the generality of readers.

It appears that we are indebted to Dr. Odier of Geneva, for the original introduction of the oxide of bismuth into the practice of physic. So early as the year 1786, he published a paper on the subject in the *Journal de Médecine*. But prejudices arising against this mineral, which have since been proved to be wholly unfounded, prevented its gaining ground, as a medicine, on the continent of Europe.

The first notice of its being employed in Britain, as far as we have been able to ascertain, is in the *Med. & Phys. Jour.* for July, 1799. It is mentioned by the editors of that work, as a medicine either neglected or forgotten, "though," say they, "it is stated to be a powerful remedy in spasmodic pain of the stomach and bowels, particularly if it arise from organic debility, or a relaxed and emaciated constitution. One of our correspondents affirms, that he has lately prescribed it in two or three cases of the above nature, not only with apparent, but

with permanent good effect, insomuch, that the cramp of the stomach, which had usually returned once or twice in the month, did not trouble his patients for six and twelve months together."

To Dr. Marcet, a physician of Guy's hospital, the credit is certainly due, of reviving this medicine; and, perhaps, of establishing its reputation. In the year 1801, he read before the London Medical Society, a paper which contained several decisive cases of the utility of the oxide of bismuth, and three years afterwards, on the publication of the paper, he remarks in a note, "I have had since, frequent opportunities, at Guy's hospital, of trying the oxide of bismuth in spasmodic affections of the stomach, and those trials have fully confirmed the opinion which I formerly gave of the utility of this medicine." Confidence in the medicine being thus inspired, it now crept into the practice of many of the distinguished physicians of Britain, some of whom have given cases to the public completely corroborating the preceding statement of its efficacy.

It would be inconsistent with the limits of our work to insert the whole of the evidence which is comprised in the essay before us, respecting the medicinal virtues of the oxide of bismuth. The cases published by Dr. Marcet and Dr. Bardsley, have been well abridged by our author. These abstracts, and the result of some additional cases which came under the care of Dr. Moore himself, or his medical friends, are, indeed, the only portions of this valuable dissertation which we can present to our readers.

*Abstract of Dr. Marcet's Cases.*

Dr. Marcet prescribed the oxide first in the case of a woman, who, probably from her sedentary occupation, had for two months preceding been afflicted with a very troublesome dyspeptic affection, where no matter, either solid or fluid, was taken into the stomach, without exciting pain at the pit of the stomach, followed by sickness and vomiting; "in short," says Dr. M. "it appeared from her account that only an extremely small portion of the nourishment which she took could be transmitted to the alimentary canal." This patient, for a month,



made use of such tonic medicines as were thought best adapted to her complaint, without deriving from them any sensible benefit; but, on the contrary, she had become thinner than when she had first applied for relief.

"Dr. Marcet having at this time procured a supply of the oxide of bismuth, ordered that she should take five grains of it three times a day, with fifteen grains of the compound powder of gum tragacanth. At the expiration of a week from the time she began its use, she said, "she was almost well," but begged to have "some more of the powders that had produced such remarkably good effects." At the end of another week she declared herself free from complaint; but for fear of a relapse she continued to take the powders a few days longer.

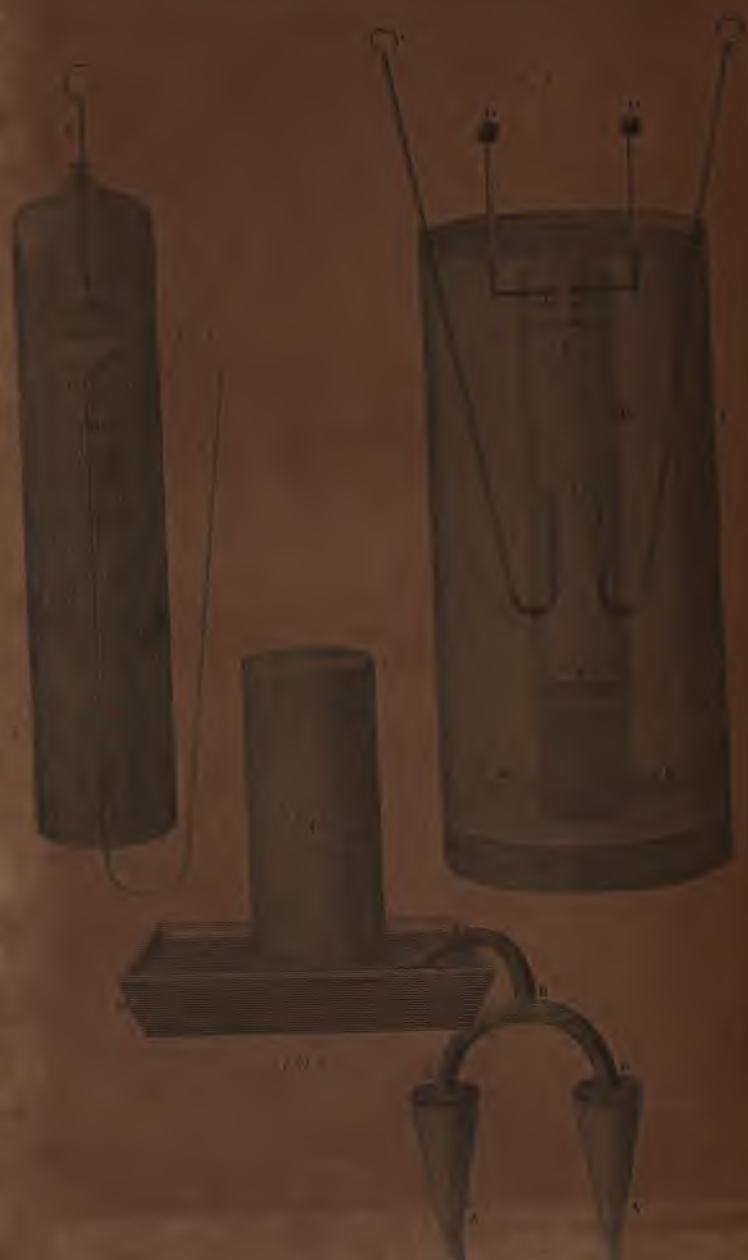
"A girl of seventeen, who "laboured under a chlorosis, the most distressing symptom of which was a violent pain in the region of the stomach," was the second patient to whom Dr. M. gave the oxide. For this patient were ordered pills of aloes and myrrh, from which no effect being observed, in a few days pills of the sulphate of iron with extract of gentian were also given. This course was continued for eight days, during which time the gastrodynia had increased; when twenty grains of the compound powder of bismuth, prepared as in the preceding case, were directed to be taken three times a day, she being at the same time ordered to omit taking any other medicine. On the third day she was much better, though there still continued some uneasiness in her stomach. The compound powder of bismuth was increased to half a drachm three times a day. On the sixth day, on account of the appearance of some other dyspeptic symptoms, she was directed to take, in addition to the oxide, a bitter infusion, with a few drops of aq. ammonizæ, twice a day. On the tenth day, being much better, the dose of the powder was reduced to twenty grains. On the twenty-fourth day from the time she commenced the use of the oxide, she was quite free from complaint, and discontinued taking any medicine.

"In the third and fourth cases in which Dr. Marcet gave this medicine it did not effect cures; but of these cases he observes, "the two instances in which it has failed, the affection, which was at first suspected to depend upon a spasm of the sto-





Fig. 1. Apparatus for the experiment.



mach, has since appeared to be of a complicated, and probably of a very different nature."

"A man fifty years of age afforded Dr. M. the fifth case. He complained of having a "most painful sensation in his præcordia, attended with sickness, great giddiness, but not vomiting." This pain, which attacked him every day after dinner, continued four or five hours, and was so violent he could hardly stand; "he had been subject to this complaint for some years past, but for six weeks it had been more constant and severe than ever before." He was directed to take twenty grains of the compound powder of bismuth three times a day. After having taken twenty-four of the powders he was perfectly free from complaint; and six months afterwards his physician heard of his having until that time continued quite well.

"The last case related by Dr. M. was that of a servant girl in his own family. She laboured under amenorrhœa, and was at the same time afflicted with a violent pain in the region of her stomach, attacking her every morning, and sometimes continuing until the afternoon. She made use of sulph. of iron, and other tonics, but the pain continued unabated; she was put upon the use of the compound powder of bismuth, in the dose of twenty grains three times a day, and in three days she was cured of the complaint."

*Abstract from Dr. Bardsley's Cases.*

The first patient for whom Dr. B. prescribed the oxide of bismuth was a man who had become dyspeptic, probably from the intemperate use of ardent spirits. He was troubled with gastrodynia, frequent eructations, heartburn, griping, and alternate costiveness and diarrhœa. A great variety of tonic medicines were given without affording any permanent relief. He twice became better, though at no time free from complaint, and each succeeding attack was more severe than that which had preceded it. Dr. B. at length having met with Dr. Marcet's paper, prescribed five grains of the oxide, with twenty-five grains of gum tragacanth,\* to be taken three times a day. This

\* Dr. Marcet's compound powder of bismuth consisted of one part of the white oxide, with three parts of the compound powder of tragacanth. Dr. Bardsley's compound powder contained one part of the oxide, with five parts of gum tragacanth.



patient found considerable relief after having taken nine doses. In nine days from the time he commenced its use he was almost well, except some degree of anorexia, with a slight diarrhœa; to relieve the last symptom gum kino and opium were given with the oxide, and in a short time he was restored to excellent health.

"Dr. Bardsley's second case, he says, "was nearly the counterpart of the foregoing one." The subject of this case, an habitual dram drinker, had been affected for upwards of a year with gastrodynia, which was always increased by eating; he was costive, and suffered much from flatulence. "The contents of his stomach," says Dr. B. "were often so extremely acid, as to excoriate his mouth and lips." After having cleansed his stomach and bowels, he was ordered to take half a drachm of the compound powder, composed, as before stated, of five grains of the oxide, with twenty-five of gum tragacanth, three times a day. On the fifth day he said he was free from sickness and gnawing pains at the stomach, from which he had not suffered since the second day of using the remedy; and he expressed a firm persuasion, that the powders would entirely cure him.

"The remedy was continued, and a few grains of rhubarb and myrrh were also directed to be taken at bed time. This plan was persevered in for about sixteen days, when there was no longer any recurrence of spasm, flatulence, or acidity.

"He took after this, for a few days, a slight preparation of bark and bitters, when he was discharged cured.

"The third case, which afforded an opportunity of using this remedy, terminated fatally. But I conceive by no means to the discredit of the medicine used for its relief, as the disease was in its nature desperate, and probably unsusceptible of being cured by any medicine. This patient had long suffered from dyspepsia, which was supposed to be connected with anomalous gout. He had formerly been addicted to the immoderate use of ardent spirits, but for some time preceding had been temperate. He complained of a fixed tormenting pain at the pit of his stomach, which gradually descended to the naval; the spasms, which generally attacked him after eating, were commonly removed by the contents of the stomach being

thrown up in a very acid state. The bowels were rather costive. Some incurable organic affection of the stomach was suspected to exist; antispasmodics, however, were administered, both combined and separately, without producing any effect.

"Bismuth was then prescribed, with a view of alleviating the spasmodic affection, and preventing the formation of acid in the stomach; and these intentions it completely fulfilled.

"This patient took, as the others had done, half a drachm of the compound powder three times a day, and gradually increased the dose to two scruples; the spasms were subdued, and the general dyspeptic symptoms much relieved in the course of a week. But after continuing for three weeks better, he was suddenly attacked with hepatitis. He recovered from this attack, but only to linger out a few weeks in a hopeless condition.

"It was found, on dissection, that the pylorus was completely indurated and scirrhus, forming a tumor somewhat of a globular form, at least four inches in circumference; the liver also exhibited evident signs of disease. Dr. Bardsley observes of this case, "it may readily be conceived, that nothing beyond a palliation of the more urgent symptoms could have been expected from medicine. The oxide of bismuth answered this purpose, probably, by diminishing the irritability of the stomach, and increasing its tone, and thereby correcting a depraved secretion, and mitigating spasm."

"The subject of Dr. B.'s fourth case had, for more than a year, been troubled with pyrosis, and for a short time had suffered from gastrodynia. His disease was most probably occasioned by his sedentary occupation as a weaver, and his former habits of intemperance: his pain was fixed and dull, not of the spasmodic kind; the quantity of acid discharged from his stomach, in a watery form, was greater than in any of the former cases.

"The stomach having been cleansed by an active emetic, and the bowels emptied by castor oil, he began taking twenty grains of the comp. powder three times a day. Few doses had been taken before the gastrodynia was relieved; the dose was gradually increased to forty grains; and, in the course of a fortnight, he reported himself free from pyrosis, and every other troublesome symptom.



"The next subject of experiment was a house-painter, who had, for many years, been subject to pains in his bowels, resembling colic; and having changed his occupation, was seized every day after dinner with excruciating pain in his stomach, which terminated on its contents being thrown up in an undigested state: he was much reduced in flesh and strength, and had a cadaverous aspect.

"The first passages having been cleansed, half a drachm of the compound powder of bismuth was directed to be taken three times a day; and a blister was at the same time applied to the region of the stomach. On the fourth day he had taken nine of the powders, and had found from them great relief. By the ninth day the spasms were completely removed, and, without the aid of any other medicine, in the course of a fortnight more he was discharged perfectly cured.

"In the last case related by Dr. B. the patient had been afflicted for two years, with a heavy pressing pain in his stomach, attended by a sense of constriction of the œsophagus; he had also acid cructations, the matter ejected being sometimes so corrosive as to blister his palate and lips. He was frequently awakened in the night by a spasmodic affection of the stomach, accompanied by a violent pulsation about the umbilical region. A moderate gratification of his appetite always increased his pain and sickness.

"The doctor confesses in this case his hopes of success were not very sanguine, as he feared the existence of some organic disease of the stomach; the compound powder, however, was prescribed, half a drachm three times a day; a small blister was also directed to be applied to the region of the stomach. On the sixth day of taking the medicine he had already experienced great relief; the stomach could now bear a moderate portion of food without much uneasiness, and no longer ejected its contents in an acid state; the spasms had also abated in frequency and duration. His appetite, however, being still languid, an infusion of bark and colomba was directed to be taken, in addition to the bismuth. On the ninth day his appetite and strength were much invigorated, but he still had some pain at the stomach, where another blister was ordered to be applied.

"By the fifteenth day the tone of the stomach was com-

pletely restored. He continued a few days longer to take the oxide; when, in consequence of a sudden attack of pain in the umbilical region, attended by considerable fever, he was directed to discontinue taking this medicine, to lose blood, and to take a cathartic. This paroxysm continued but a few hours, and the next day he resumed his former plan, by which he was, in a few days, restored to health.

"I feel happy, says Dr. Moore, in having it in my power to relate the following cases, wherein I have had an opportunity of witnessing the effect of bismuth in relieving spasmodic pain, with some other troublesome affections of the stomach; and I have no hesitation in declaring, that it has equalled my most sanguine expectations."\*

*\* Dr. Moore gives the following Directions for the Preparation of the White Oxide of Bismuth:*

The bismuth to be dissolved should be previously reduced to powder in an iron mortar. Let three parts of nitric acid for one of bismuth be diluted with an equal weight of pure water. To this menstruum, contained in a glass vessel, add the bismuth at intervals, and let it stand till it is all dissolved. Let the clear solution be decanted from the sediment, (a small quantity of which will always be found) and a few ounces of it be poured into a glass vessel capable of containing half as many gallons as there have been measured ounces put in;\* the vessel is then to be filled with pure water, when a copious and perfectly white precipitate will be instantaneously formed, giving to the liquid the appearance of milk,

After this has subsided, the clear fluid must be decanted, and fresh water thrown on the precipitate to wash it. This operation must be several times repeated, until no acid taste is discoverable in the decanted water.

This precipitate, which is the pure white oxide of bismuth, should be suffered to dry without heat, or indeed light, for the attraction between bismuth and oxygen is so weak, that if the oxide, while drying, be exposed either to a very moderate artificial heat, or the direct rays of the sun, it parts with a portion of its oxygen, and loses its whiteness.

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\* No glazed earthen ware vessel should be employed for this purpose, on account of the lead so frequently used in glazing them. A large wine flask, or demijon, as it is more generally called, capable of containing four or five gallons, will be found a very convenient vessel. Both Fourcroy and Murray observe, that the precipitate is a purer white the more water there has been used in its formation; and I have found that if the quantity of water used be much less than has been directed, the supernatant liquid, after the precipitate has subsided, still contains a considerable quantity of the oxide, capable of being thrown down by the addition of more water.



*New-York, Aug. 30, 1810.*

"Case 1st. C. a mulatto woman, aged thirty-six, has, for the last seven months, been dyspeptic, during which time I have occasionally visited her, and given her medicines; she is better than she was, but still complains of being troubled every morning, about four o'clock, while still in bed, with sickness, preceded generally by coughing a few times, when she throws up from her stomach a watery fluid, seldom very acid. She assures me, she has not been free from these attacks scarcely for a single day during the last seven months. She has more than once taken emetics, and succeeded their use by infusions of gentian and orange peel, without deriving from them much benefit.

"Although I did not doubt that many cases might be found in which bismuth was calculated to do more good than in the present one; I, nevertheless, felt a desire to give it a trial, and accordingly directed her to take, three times a day, five grains of the oxide, with fifteen of gum arabic and sugar.

"Sept. 5th. She is better, and tells me, she has vomited but twice since she commenced taking the powders. She has taken seventeen doses of the medicine, and has but a single one remaining.

The oxide prepared in this way is purer and whiter than that formed by any other process. I have precipitated it from its solution, both by potash and ammonia, but in neither case was it so pure a white; and, indeed, the administration of the oxide, prepared by means of an alkali, might be attended with danger, in case the bismuth had been alloyed with other metals, which would also be thrown down by the addition of an alkali to the solution.

Whatever reason we may have for thinking the bismuth we make use of impure, (which can scarcely be the case without being discoverable from its appearance) still we have no reason to fear that the oxide will partake of the impurities of the metal which has been employed, if it be prepared in the manner which has been directed; for Nicholson assures us, "the precipitation of the nitrous solution by the addition of water is the criterion by which bismuth is distinguished from all other metals."† And, indeed, if our only object were to separate the bismuth from its alloys, in a state of purity, no better means could be employed to obtain our end than those which have been recommended for preparing the oxide.

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† Nicholson's Chem. Dict.

" 9th. She has very little sickness, but still throws up in the morning a small quantity of fluid from her stomach. I gave her to-day some more of the oxide, with directions to take it as before.

" 12th. Since she recommenced the use of the remedy she has had no return of either sickness or vomiting.

*New-York, Sept. 9, 1810.*

" Case 2d. Mr. L. aged forty-five, has, for the last three years, been afflicted with cardialgia, attended by extreme flatulence; he has at no time during this period been free from these complaints. He has frequent acid eructations, which he says are also very acrid, occasioning his throat to feel as though it had been scalded; sometimes also, though rarely, he is sick at his stomach, when he vomits up a matter, having the colour of coffee grounds. He has a number of times taken emetics, and, since the commencement of his complaint, has been in the daily habit of taking magnesia, from the use of which, although he has sometimes thought it afforded him temporary relief, it is pretty evident, from the continuance of his disease, that very little permanent advantage has resulted.

" During the last winter and spring Mr. L. had occasional attacks of severe gnawing pains at the pit of his stomach, which, warm external applications would, commonly in the course of fifteen or twenty minutes, relieve. Throughout the summer he has been generally free from these spasmodic pains; but for the last eight or ten days they have attacked him much more severely; usually returning two or three times in a day. These attacks are very violent, occasioning great anxiety, succeeded by insufferable pain, throwing him immediately into a profuse perspiration. The warm applications, from the use of which he had, on former occasions, found benefit, at this time afford him not the slightest relief.

" For the last two months he has been making use of very strong bitters, and I think it probable he has injured his stomach by the large and frequently repeated doses which he has taken. A very sparing use of either fermented or spirituous liquors never fails to increase his cardialgia. His bowels are quite regular, and his pulse feeble.



"Mr. L. at this time sent for my father, who directed him to take, three times a day, five grains of the oxide of bismuth, combined with an equal weight of refined sugar, and twice its weight of starch.

"Sept. 11th. He has had no return of the pain in his stomach since he commenced the use of the oxide, but feels well enough to-day to take a ride into the country.

"Monday morning, Sept. 17th. He still continues free from the spasmodic pain of his stomach; his cardialgia, although not well, is better than it has been hitherto. He has taken none of the bismuth since the morning of the 13th, (Thursday last) when he took the twelfth, and only remaining dose he had, and, being out of town, he had it not in his power to procure another supply until his return to the city to-day.

"Monday evening. He had this afternoon a return of that distressing anxiety which has always hitherto preceded his attacks of pain in the stomach. Fearing he was about to have another attack, and placing full reliance on the oxide, from his experience of its efficacy, he sent immediately for a supply of it, but before the person returned with the medicine he had taken forty drops of laudanum; he, however, took directly a dose of the oxide, and the pain did not, as he had expected, succeed; but, as he had taken laudanum previously, we should not be justified in attributing to the bismuth alone the disappearance of his unpleasant symptoms.

"Wednesday, Sept. 19th. Our patient has not since had any return of the spasms; the cardialgia is infinitely less troublesome. I gave him to-day twenty-four doses of the powder, each containing six grains of the oxide, directing him to take one three times a day.

"Sunday evening, Sept. 23d. Mr. L. is now free from all complaint. His heartburn, which was very troublesome until he began taking the bismuth, does not at present appear, unless it be brought on by the use of wine or brandy; he can, however, drink moderately of West India spirits or gin, without experiencing the least inconvenience from so doing. On Thursday last, after riding twelve or fourteen miles, he began to feel some pain in his stomach, but was immediately relieved on taking his customary dose of the oxide.





*Fig. 1. Apparatus for the study of the effect of the electric current on the heart.*



*New-York, Sept. 24, 1810.*

"Case 3d. Mr. B. the subject of the following case, has been for three or four years dyspeptic; during which time he has almost continually suffered from cardialgia and pyrosis, and very frequently severe colic pains. The fluid which he threw up from his stomach was, at times, he said, so extremely acid, that it felt as if it were cutting his throat while passing through it. He has been long in the habit of using alkaline solutions, absorbents, &c. but without effect.

"Mr. B. has, at my request, very obligingly drawn up a statement of his own case, the substance of which I shall give nearly in his own words.

"I have been for several years troubled with acidity on my stomach, a watery fluid running from my mouth, and an unpleasant sensation, or dull pain at my breast, frequent colic pains; sometimes a cough, which was always accompanied by an unpleasant taste: a great heaviness and inclination to sleep, with an aversion to motion. My appetite has always been good, and sometimes, indeed, voracious; but even at these times my stomach would be often disgusted by the first mouthful of food I swallowed. When the heaviness came over me, I felt a sensible diminution of my strength and spirits. I have taken soda, magnesia, chalk, and rhubarb, but have found no relief from them. On the 16th of September I called on Dr. Moore, and obtained from him a few powders, of which I have taken three a day, and have not, for the last five days, felt any symptoms of the complaint.

*September 24, 1810."*

"Mr. B. took fourteen doses of those powders, containing, altogether, one drachm and ten grains of the oxide. He assures me that he has never felt better than he does at present, and says, that if the powders produce a permanent cure, it will be one of the most happy events of his life.

"Sept. 29th. I saw our patient this day, when he informed me of his having continued thus far perfectly free from all indisposition.

"Dr. Stringham has lately been giving the oxide of bismuth



to a lady labouring under menorrhagia, attended with extreme debility, who has experienced from it astonishing relief.\*

“ Dr. Post, professor of anatomy and surgery in Columbia college, to whom I, a short time ago, gave some of the oxide, has since informed me of his having prescribed it for the relief of some dyspeptic complaints, and he expresses entire satisfaction with the effect which it produced. He particularly mentioned his having given it to a lady during gestation, who, not only at such times, but generally, suffered much from acid in the stomach. She had derived but little benefit from the medicines ordinarily prescribed in such cases; but was very shortly relieved on taking only a small quantity of the oxide.

\* I very much regret that, owing to some particular engagements of Dr. Stringham, it has not been in his power to furnish me, as he had intended, with a particular statement of this case.

## INTELLIGENCE,

## MEDICAL AND PHILOSOPHICAL.

**A REPORT** having been lately circulated that Dr. Jenner himself was beginning to entertain some doubts of the efficacy of vaccine inoculation in securing the system against the influence of small-pox, we presume the friends of vaccination will be gratified by the perusal of the following letter addressed to the secretary of the cow-pox institution in Dublin.

Dear Sir,

Your obliging letter of the 2d inst. inclosing the annual report of the cow-pox institution in Dublin, has just reached me. The former letter you allude to, has not yet been delivered. It is with the greatest pleasure I perceived the rapid increase of vaccination in your metropolis, and the uninterrupted success that has attended the practice, at once a proof of the skill, industry and attention of the medical officers, for whom I beg leave to make my most grateful acknowledgments.

And now, sir, a few remarks on the very extraordinary communication you have made to me respecting the small-pox. It has been one of the usual devices of the enemies of the vaccine, almost from the time of my first making it known, to represent me as having lost my confidence in the efficacy of the vaccine, or at least, that I was wavering on the subject. I have, however, with the aid of my nephews, have vaccinated upwards of 30,000 persons, little short of 30,000, without one single instance of accident, or failure, that ever reached my ears, how could I entertain such an absurd idea? Or could I ever even thought of inoculating for the small-pox, while I hold that practice in abhorrence, and condemn it both publicly and privately. Believe me, the whole story you relate to me is an entire fiction, without the faintest shadow of foundation. Never, from the commencement of my experiments, to the present moment have I used a particle of variolous matter, except for the purpose of putting some of those to a test, on which I made my first trials. For some years past I have relied wholly on the vaccine lymph, for



testing those on whom any material irregularity appeared in the progress of the pustule.

*Berkely, Feb. 19, 1809.*

EDWARD JENNER.

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The late British publications announce the discovery of a new and important medicinal property in the rectified oil of turpentine. It is given in doses of from half an ounce to two ounces, fasting, for the removal of *tenia*, a parasitical genus of animals, which have hitherto frequently baffled all attempts to dislodge them from the alimentary canal. It generally excited an agreeable warmth in the stomach without any degree of sickness or disposition to vomit. The patients however, complained of giddiness, pain in the head, and sometimes of thirst, accompanied by a white tongue, and a warm state of the skin. It would appear that this remedy which seems to act as a poison to the worms, is most effectual when given by itself, and that the distressing symptom of strangury is more likely to occur, if it be employed in small doses, than when exhibited in sufficient quantity to insure its purgative operation. It may therefore be a question to be determined by future experience, whether combining the oil of turpentine, or rather following up its employment with some active cathartic medicine, might not answer an advantageous purpose. There is reason to hope, that this remedy will also prove efficacious in expelling the other worms which infest the human body, while from the quick relief which it gives to horses affected with dry gripes or spasm of the intestines, it may perhaps be worth trying in such cases of colic as do not readily yield to the ordinary modes of treatment.

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“Mr. Astley Cooper continues to perform lithotomy by means of the knife alone, with constant success; and we hear that this method has been lately adopted in the Exeter hospital. The prejudice, which this deviation from the accustomed routine will probably encounter, may be diminished by our observing, that surgeons are by no means unanimous in their preference of Hawkins's method. “The cutting gorget,” says Richerand, “is employed almost exclusively in England, Italy, Germany, and the north of Europe; the instrument of Frère Cosme, used by individuals in all countries, generally adopted by the

French." (Tom. III. p. 515.) The instrument itself, and the manner of operating with it, are well described in this work. The apparatus, consisting merely of a staff, knife, lithotome caché, and forceps, engages our commendation by its simplicity. The situation and direction of the external incision, as advised by this author, are the best calculated for the purposes of the operation: commencing on the left of the raphé, at about ten lines in front of the anus, it should be continued downwards and backwards, between the anus and the tuberosity of the ischium, for an extent of two or three inches. When the fat and muscles covering the urethra have been divided, the canal laid open in its membranous portion, the lithotome introduced into the bladder, and the staff removed, the former instrument should be so placed, that its cutting edge may be directed obliquely downwards and towards the left, in correspondence with the course of the external incision. It should then be drawn out horizontally, by the right hand grasping the handle, so as to make the blade quit its sheath. In this way an incision is made into the urethra and neck of the bladder, directly opposite to the cut in the integuments, and the forceps, guided by the fore finger of the left hand, introduced for that purpose into the wound, are carried in a straight direction into the cavity of the bladder. Such is the French operation, which, if properly performed, seems to be very well calculated for attaining our objects. The use of the finger in examining the course and size of the incision, and the position of the stone, and afterwards in guiding the forceps; the place of the cut in the integuments; and the course of the opening in the bladder, are circumstances of particular advantage in this plan. The following are the comparative merits of the operation by the lithotome caché, according to the opinion of Richerand. "By cutting as it is drawn out, and not when it enters, a false passage cannot be made, and the neck of the bladder cannot escape division. If it slips from the groove of the staff, the blunt end is resisted by the cellular substance, and thus we are warned to withdraw the instrument, and introduce it again into the groove. The instruments, which cut as they go in, make way for themselves; and the mischief is done before we perceive that they have taken a wrong direction. Where the incision is made



before backwards, the parts yield and elude the instrument: a narrow cut and a consequent difficulty in removing the are the consequences. When the lithotome is employed, neck of the bladder cannot escape the action of the blade, it must be divided in an extent corresponding to the degree of its aperture." p. 536, 537.

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In London the two charities for the relief of persons affected with diseases of the eyes, have become firmly established, and have reached that extensive utility, which must very highly gratify the benevolent feelings of their patrons. The extraction of the cataract is practised at one of these institutions, and with greater success than it has ever met with, in any other hands. We cannot state the precise numbers of the fortunate and unsuccessful results, as we have no printed report to refer to; but we know, that the latter are so few, that the operation may be deemed certain of its object, unless some unfavourable circumstance is present. We need not state how complete a contrast to this representation would be exhibited in the fate of patients operated on in general hospitals. We cannot help indulging a wish, that the vast opportunities of practice, afforded by these charities, should be employed for the purpose of settling the hitherto disputed question, whether depression or extraction be the preferable operation. Among writers, the greatest authorities are decidedly in favour of the former; while the celebrated oculists of this metropolis practise the latter exclusively. This apparent contradiction will surprise us less, when we reflect that the practice of the oculist depends entirely for its existence, as a distinct branch of the profession, upon this single operation; and, that if couching were introduced instead of it, any given surgeon would be able to cure cataracts as well as the most expert oculist. From these considerations we had great pleasure in hearing that the late Mr. Saunders, surgeon to the London infirmary for the eye, had practised a modification of couching in certain cases, and particularly that he had applied his method to very young subjects, in which extraction is altogether impracticable. This gentleman was on the point of communicating to the public his experience on the above mentioned subject, when an apoplectic attack suddenly removed

him from a situation at which the exertion of his talents was not less beneficial to others, than honourable to himself; just when he was beginning to receive the well-deserved reward of public fame and emolument. While we deplore the loss experienced on this occasion by the public and the profession, it is satisfactory to learn, that Mr. Saunders's manuscripts are nearly ready for the press, and that his work will appear without delay. In the mean time we may state, as a matter of general report, that the operation performed by Mr. S. on young children, consisted simply in puncturing and disturbing the cataract, without attempting to depress it. The opaque substance, when its natural connexions are thus destroyed, will be removed by absorption. Our readers will be pleased to see Mr. Saunders's own statement of his proceedings; which we extract from a letter published in the report of the London infirmary, 1809. "In addition to the observations made on the former reports, and equally applicable to the present, there is one point on which I must beg the indulgence of expatiating; I mean the adaptation of an operation on the cataract to the condition of childhood, by which I have successively cured, without a failure, thirty-one persons born blind, many of them in infancy, and one even at the early period of two months. Reserving, for another occasion, the communication of the method which I pursue for the cure of very young children, I shall no farther compare it with extraction, than by observing, that extraction is wholly inapplicable to children, or only fortuitously successful. Those who on all occasions adhere to this operation, and have never turned their thoughts towards the application of means more suitable to this tender age, have been obliged to wait until the patient has acquired sufficient reason to be tractable: otherwise, when they have deviated from this conduct, the event has afforded little cause of self-congratulation. How great the advantage of an early cure, is a question of no difficult solution. Eyes originally affected with cataracts contract an unsteady and rolling motion, which remains after their removal, and retards, even when it does not ultimately prevent, the full benefit of the operation. A person cured at a late period cannot overcome this awkward habit by the utmost exertion of reason, or efforts of the will. But the



factions of the infant are instinctive. Surrounding objects attract attention, and the eye naturally follows them. The management of the eye is therefore readily acquired, his vision rapidly improves, and he will most probably be susceptible of education about the usual period."

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Mr. Albers, of Bremen, having examined the eye of the codfish, *Gadus Morrhua*, L. finds that the sclerotica is composed of two coats; the outermost of which is membranous, while the innermost is horny, and divisible into several layers. The innermost of these layers is coated with a substance resembling spermaceti, which forms little cysts, containing water in their intervals. This separates the sclerotica from the outermost silvery coat of the choroides, which by maceration becomes divisible into two layers. The intermediate coat of the three principal ones, that occupy the place of the choroides in the eyes of fishes is composed in the cod of a retiform vascular texture, the trunk of which issues from the horse-shoe shaped tubercle peculiar to fishes, and the use of which is so difficult to explain. Some anatomists have taken this tubercle for a gland, others for a muscle; but it appears from the minutest research to be a convolution of larger bloodvessels, and Mr. Albers considers it as a sort of reservoir for the blood, a kind of rete mirabile.

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In the 10th vol. of the Asiatic Researches Dr. Rene has given an account of the male plant, which furnishes the medicine generally called *colomba root*. This plant is indigenous on the eastern coast of Africa: a fact we believe never hitherto ascertained, although the Portuguese export the root in very considerable quantities from the Mosambique. An offset taken to Madras, and there planted, produced only male flowers, so that the genus is still uncertain.

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The conclusions drawn by Mr. Davy in his late publication on the muriatic acid will serve to extend and enlighten the theory of chemistry to a greater extent than any of the brilliant discoveries formerly made by this illustrious chemist. They are as follows.

1st. That the oxymuriatic acid is a simple substance, which may be classed in the same order of natural bodies, as oxygen gas; being like oxygen determined to the positive surface in voltaic combinations, and like oxygen, combining with inflammable substances, producing heat and light.

2dly. That its combinations with inflammable bodies are analogous to oxides and acids in their properties and powers of combination, but they differ from them in being, for the most part, decomposable by water.

3dly. That hydrogen is the basis of the muriatic acid, and oxymuriatic acid its acidifying principle.

4thly. That the compounds of phosphorus, arsenic, tin, &c. with oxymuriatic acid, approach in their nature to acids, and neutralize ammonia and other acidifiable bases.

5thly. That the combination of ammonia with phosphorus, acidified by oxymuriatic acid, is a peculiar compound, having properties like those of an earth, and is not decomposable at an intense red heat.

6thly. That the oxymuriatic acid has a stronger attraction for most inflammable bodies, than oxygen; and that on the hypothesis of the connexion of electrical powers with chemical attractions, it must be highest in the scale of negative power, and that the oxygen, which is supposed to exist in oxymuriatic acid, has always been expelled from water or oxides.



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THE  
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No. III.

SELECTED PAPERS.

*Case of Hydrophobia, wherein the operation of Tracheotomy was performed; and an account of the appearances on dissection.*

By HENRY OLDKNOW, Surgeon, Nottingham.

From the Edinburgh Medical and Surgical Journal, for 1809.

ON the 25th of October last, William Hill, aged forty-five years, by trade a tailor, had a portion of the scrotum torn away by the bite of a dog; at the same time he received a contusion on the thigh, close to the scrotum; and, in attempting to separate the jaws of the animal with his hands, the point of a tooth penetrated the back part of one finger of the left hand, making a small wound, similar to the puncture of a small nail. There being a general alarm about mad dogs at this time, and some unpleasant symptoms existing in this dog,\* it was thought pru-

\* It was thought at this time the dog was not mad; he ate and drank heartily, but very soon rejected the food from his stomach; he appeared healthy, and showed no signs of indisposition, except sickness after meals, it was observed to be out of temper, the day before having bit the boy who brought him his food, and who generally had the care of him. This, however, was not much noticed, as he frequently had been observed to tease the animal at these times, by pointing at him, &c. nor would the boy submit to have any thing done to him, being confident the dog ailed nothing; however, it was thought proper to keep him confined. The day following, wishing to



dent to take the necessary precautions; consequently the edge of the lacerated scrotum (the wound being about the size of a shilling, and exposing the tunica vaginalis testis) was pared away and cauterized; the contusion on the thigh and the punctured wound of the finger were cauterized only; afterwards rags wet with the aqua ammonia pura were applied over the cauterized parts, until considerable inflammation was excited; so that, in a few days, a large slough was cast off from the thigh, and a small one from the punctured finger, each according to the extent of surface on which the caustic was applied. The wound on the finger suppurred very little, and soon healed; that on the scrotum, and the contusion on the thigh, suppurred copiously, and healed in about a month.

On the 10th of December, forty-six days subsequent to the bite, I was desired to visit this man, who I found complaining of sickness, and a great oppression at the stomach; flatulence, frequently sighing, moaning, and breathing short; having great distress of countenance, and a sensation of constriction about the upper part of the throat; great difficulty in swallowing liquids, the sight of which agitated him much, and produced, to all appearance, a spasmodic action of the diaphragm and muscles of the glottis, similar to what is observed in hysteric paroxysms, and which was greatly increased in the attempt to swallow; he had equal difficulty in swallowing solids. On examining the wounds, they were found to be healed; that on the thigh was in part covered with a scab, which easily separated, without tearing the new skin formed underneath; nor did the cicatrices appear unhealthy or inflamed. He had, however, complained, as much as four days before, of a soreness, or, to use his own expression, of a sensation of roughness

remove the dog, a very foolish and unsuccessful attempt was made to get him into a bag, to convey him with more safety, and in the attempt the collar slipped over his head. Fearful now of some mischief, it was agreed he should be shot. The ball missing him, the report of the pistol alarmed him so much, that he sprung to the door of the yard he was confined in, and, making his escape, seized this poor man, who stood directly in his way, and did not relinquish his hold until stunned by a blow on the head, and was afterwards killed. The body was opened, but no diseased appearances presented themselves; his stomach was completely filled, even to distention, with a quantity of hay he had eaten in the kennel.

in the skin, about the middle of the humerus, with a prickling numbness, and sometimes tremor, of the left hand and arm.

It appeared, on inquiry, that the first symptoms of constitutional derangement were noticed the day preceding, during dinner, at sight of which he felt great disgust; and it was with difficulty he could be prevailed on to eat, complaining of nausea, headache, and oppression at the precordia; and, in the evening, wishing to take some warm ale he saw preparing for another person, it had no sooner touched his lips than he became greatly agitated, and could not swallow more than two or three draughts. His rest was disturbed during the night, and he complained of a sensation of suffocation when lying down, so as to oblige him to keep an erect position. Notwithstanding these symptoms had existed several hours, I was not sent for until the following morning about nine o'clock. An emetic was directed, which he took with reluctance, from the horror he felt at the sight of fluids. However, he did hastily swallow it, after many attempts, and was convulsed for about half a minute; its operation was distressing, from not being willing to drink with it. A quantity of yellow fluid was evacuated, but without relief to the stomach. Two blisters were applied to the external fauces, and two ounces of strong mercurial ointment directed to be rubbed into the inside of the legs and arms. A consultation was appointed at twelve o'clock, at which many professional gentlemen were present, and it was further directed, that a blister should be applied to the scrobiculus cordis, another to the wound on the thigh, and one to the inner side of the arm affected; ten grains of musk, ten of carbonate of ammonia, and one of opium, to be given in the form of a bolus, every three hours, and six ounces of strong mercurial ointment to be well rubbed over the whole surface of the body. It was likewise directed, that Webb's medicine, recommended by Mr. Blané, in the Medical and Chirurgical Review, should be administered, which was prepared in the form of a decoction, with milk, because the poor man now found less difficulty in swallowing fluids than solids, although the former, when taken, produced so marked an agitation of the whole frame; indeed, so incapable was he of taking any



thing of substance, that the boluses were obliged to be dissolved whenever he took them.\*

At six o'clock in the evening we met again, and found our patient much worse; every symptom of the disease had increased to an alarming degree. He appeared frightened when any person approached him; spat up a considerable quantity of frothy mucus; and the constriction of the throat before spoken of, (the situation of which he described by pointing to the *pomum adami*,) became so distressing, that he requested the window of the room might be opened to admit more air. From this circumstance, it was proposed that the operation for tracheotomy, as suggested by Dr. Rush, should be resorted to, whenever the sensation of suffocation should become so urgent as to justify such an attempt for the preservation, or rather the prolongation of life. Antispasmodic clysters, with laudanum, were directed to be administered every three hours. At twelve o'clock at night, an opening was made into the trachea, between the thyroid gland and the sternum, but without affording any relief. The difficulty of breathing gradually increased, until about half an hour before his death, when he became suddenly relieved; he now drank, and asked for a basin of water to wash his hands in, both of which he did without expressing any uneasiness, but immediately after which, sunk into a lethargic state, and died about four o'clock in the morning. It was observed by those present at his death, that, from the quantity of mucus collected about the glottis, respiration would have ceased sooner had it not been for the opening made into the trachea.

Not in any stage of the disease did this poor man make violent convulsive efforts of the body, nor was he ever unconscious of what passed about him. His pulse was, throughout, small, feeble, and a little irregular, averaging from 80 to 100 beats in a minute.

\* It is a remarkable fact, and worthy of observation, that the swallowing of fluids was accomplished with comparative facility, when strong pressure was made upon the lesser lobes of the ears. This expedient was suggested by a gentleman present, who recollected to have read of its good effects in lessening the difficulty of swallowing in *cynanche tonsillaris*.

*Dissection, thirty-two hours after death.*

The body entirely free from putrefaction; inflammation of the mucous membrane of the trachea, extending from the glottis to the subdivisions of the bronchiæ, and considerable mucous secretion, similar to that spat up during life; the right lung distended with air, the left collapsed; no unusual quantity of fluid in the pericardium; the heart fatty; the left ventricle distended with black blood, the right almost empty; the diaphragm free from diseased appearance; no inflammation of the pharynx or œsophagus; the stomach greatly distended with air, but void of solid contents; considerable erythematous inflammation about the cardia; the intestines somewhat distended with flatus, but healthy; the liver healthy; the gallbladder distended with bile; the spleen, kidneys, pancreas, and urinary bladder, of a natural appearance; unusual vascularity of the pia mater and brain, the lateral ventricle containing about six drachms of fluid.

The morbid appearances of the brain are probably the consequence of a fall he had about a year previous, down a flight of stairs, from which he received a violent contusion of the head, and at the same time fractured the lower jaw.

This accident confined him to his house for many weeks; indeed, he never perfectly recovered it, being ever after subject to violent headache and derangement of intellect, whenever he committed any irregularity in living.

Another case of the hydrophobia has lately occurred in a village near Nottingham, any particulars of which I am not in possession of, except that the disease occurred about a period of six weeks after the bite, and was extremely rapid in its progress. I mention it, because the dog that bit this individual belonged to a man in this town, and was not suspected to be mad until ten days after he had bitten this unfortunate sufferer. He ate and drank heartily, showed no signs of indisposition, hunted as usual, and occasionally went into a neighbour's house, among children, without injuring any one of them; but on the morning of the tenth day, he was observed in the street, snapping at every dog that passed, and was immediately taken in and destroyed.

This is a very curious and important circumstance, inasmuch



as it tends to show, that canine madness, in its incipient stages, is very difficult of detection; and that a dog, at this period of the disorder, is capable of communicating the infection, which, I believe, is not generally understood.

Nottingham, Feb. 11, 1809.

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*Remarkable Recovery from a very extensive Wound in the Abdomen.*

By B. HAGUE, Surgeon, Ripon.

From the Edinburgh Medical and Surgical Journal, for 1809.

AUGUST 30th, 1808, ten o'clock, A. M. I went to Norton Mills, about four miles from hence, to see John Brown, aged twelve years, who had received a wound in the abdomen, by a pair of wool shears. On my arrival, which was little more than an hour after the accident had happened, I found the poor lad in a very distressing situation; the *great arch of the stomach*, and the whole of the *intestinal canal* (duodenum excepted) contained within the abdomen, having protruded through the wound. The incision was on the left side of the body, commencing at about two inches below the *scrobiculus cordis*, and extending in a straight line near four inches in length, distant from the navel two inches, so that I suppose it must have been pretty nearly in the course of the *linea alba* of that side. He was quite sensible, and had vomited so as to empty the stomach; very little blood was lost. I immediately proceeded very carefully to examine the protruded viscera, none of which were wounded, and reduced them as quickly as possible, beginning with the stomach, and following the regular course of the intestines, in the latter portion of which I distinctly felt feces, of rather firm consistence. He complained of some pain during the reduction, though not much, and expressed great relief when the parts were completely returned. I now desired an assistant to lay the palm of his hand over the wound, and make some pressure upon it, for I found, that without this, the parts would soon have protruded again by the action of respiration, which was oppressed and laborious. I brought the sides of the wound together by five sutures, beginning from above downwards, and passed the needle on

each side quite through the integuments with the peritoneum, observing great caution not to wound the obtruding intestines, by keeping my finger betwixt them and the point of the needle, in passing it through. I next applied straps of adhesive plaster, so as to produce compression, and over them a compress of linen, securing the whole with a bandage, broad enough to reach from sternum to pubes. Before I had done, he complained of feeling very faint, and I gave him to drink a little weak wine and water, which staid upon his stomach. I did not, either before the reduction of the protruded parts, or after, observe any material alteration in the state of the pulse; it was quick and rather small. I had him laid in bed upon his back, and particularly desired he might be kept in that posture. I took twelve ounces of blood from his right arm, and dissolved an ounce and half of magnes. vitr. in half a pint of broth, of which I directed four table spoonfuls to be given every two hours, and left him tolerably easy.

7 o'clock, P. M. Respiration much oppressed, accompanied with a rattling which occasioned great uneasiness. Abdomen not very painful; he had retained what he had swallowed, but had not had a stool. I administered an enema prepared with magnes. vitr.  $\mathfrak{z}$ ij. dissolved in twelve ounces of gruel, with an ounce of oil; it staid with him near five minutes, when it returned, and brought away some feces, which I judged were from the rectum only. As the injection passed up, he expressed great satisfaction, and observed, "it felt very nice." As soon as the bedpan was removed from under him, he vomited the whole of what he had taken, which had been very little, and that only fluid, in addition to the opening medicine; a lumbricus worm, which was dead, was also thrown up. Immediately after the vomiting, his breathing became more free and easy, and he was evidently much relieved by it. As he expressed so much comfort from the clyster, I waited two hours in order to repeat it: I then injected some broth only, which he observed, as before, was "very nice;" it came away almost immediately, without bringing any thing else with it: he fell asleep soon after. I left tinct. opii gtt. xxx. which I desired might be given when he awoke, if at all restless or uneasy. Pulse quick, skin hot, and he complained of thirst.



Deeming it absolutely necessary to keep the stomach as empty, and in as quiet a state as possible, I desired that very little fluid should be given him, and totally forbid solids of every kind.

31st, 8 o'clock, A. M. Had slept pretty well from the time of my taking my leave last night, till two o'clock this morning; after that he became restless and uneasy; his attendant omitted giving the opiate, as directed; pulse quicker than last night, and hard, skin very hot, tongue white, and thirst increased, respiration more oppressed, accompanied with rattling and a frequent short cough: he made no complaint of pain in the body, except a soreness of the wounded part; had made water two or three times during the night, but had no stool. I administered a clyster of a solution of magnes. vitr. &c. which very soon came away, without bringing any fecal matter with it. He had not vomited since last night, although he had been allowed more to drink than I wished. I took away twelve ounces of blood, which produced faintness, but he did not vomit. I left him betwixt nine and ten o'clock, and ordered the following mixture to be sent for immediately, *R magnes. vitr. ʒj. solve in aq. fervent. ʒvj. cola et adde tinct. sennæ ʒvj. M. detur coch. iij. 2ndâ. quaq. horâ donec alvus bis terve responderit.* I had been at home above an hour before the messenger came for the medicine, who informed me, that very soon after I had left the house, he had a stool, and before he came off, he had another: I therefore altered the direction for taking the mixture, and ordered two table spoonfuls to be given every three hours.

7 o'clock, P. M. My partner, Mr. A. Robinson, rode with me to see our patient this evening. We found him breathing with great difficulty and rattling; cough short, frequent and teasing; pulse quick and hard, and every febrile symptom aggravated since morning. Abdomen painful and swelled. The stools he had had in the forenoon were unfortunately thrown out, so that we could not see them, as was at this time most desirable; but the nurse informed us they were quite watery, without any fecal matter or smell; our hopes, therefore, of having obtained a complete passage through the bowels, as we expected to find, were by this account disappointed. He had

thrown up the medicine every time he took it. We repeated the bleeding ad deliquium, and administered an injection as before, which immediately returned without any thing else, and he vomited a considerable quantity of a dark green fluid, very like bile. On examining the bandage round the abdomen, it was found so very tense, in consequence of the distention which had now come on, that we considered it proper to unloose and slacken it, from which he experienced some relief. We left him in a great deal of pain, and in a state of considerable anxiety, though he breathed somewhat easier than before he vomited. I gave him the opiate he ought to have had last night, and we took our leave, very doubtful of his living till morning.

Sept. 1st, 8 o'clock, A. M. I had the satisfaction of finding him much better than last evening. Respiration easier, cough less frequent, and rattling not so loud; he had no more vomiting; abdomen greatly subsided, and much less painful; external heat abated, and he could bear a little pressure upon it, without complaining; pulse softer, and every other febrile symptom mitigated; he had no stool, but had made water plentifully. Our directions had been strictly followed, and he had been allowed very little to drink; the stomach, therefore, was kept nearly empty, and free from distention. I administered a clyster, which staid near a quarter of an hour, when it came away discolored, and having a strong fecal smell. Fully convinced of the necessity of keeping the stomach as empty and as quiet as possible, I still more particularly desired that he might have very little to drink; and as he still continued very thirsty, I ordered tamarinds to be taken occasionally, instead of any kind of fluid, and directed a tea spoonful of the following electuary to be given every two or three hours: R elect. sennæ  $\overline{z}$ j. ol. ricini  $\overline{z}$ ss. m. ft. elect. The blood taken away last night exhibited a highly inflammatory appearance; buff very dense, and very much contracted or *cupped*.

7 o'clock, P. M. Continues better; and had been tolerably easy during the day. My directions in regard to regimen had been strictly observed, and I found his body in an easy, relaxed, cool state. About two o'clock he had had a stool, which was shown me; it was quite fluid, but in smell and color highly



fecal, in quantity about three quarters of a pint: he had coughed less, and breathed easy, with very little rattling.—Rep. tinct. opii gtt. xxxh. s.

2d. He had slept so well the early part of the night, that his nurse omitted giving the draught. The cough and difficulty of breathing were increased this morning; pulse hard, febrile symptoms increased. He complained of pain in the left side, and thought himself, as he evidently was, in every respect worse; he had had no stool, had made water plentifully. I repeated the bleeding; in endeavouring to pass up the injection, it failed, and but very little was got up. The body was relaxed, and not painful on pressure. I ordered the electuary to be continued as before; and directed two table spoonfuls of an emulsion, prepared with oil of almonds, with a little spt. of nitre in it, to be given every three hours. He complained very much of hunger, and I gave him half a dozen spoonfuls of boiled milk, which he took greedily. I remained with him half an hour after he had taken it, and it staid upon his stomach. I desired a clyster might be given at two or three o'clock in the afternoon, if he should have no stool before that time.

7 o'clock, P. M. The clyster had been given as directed; it staid with him two or three minutes, and then brought away with it a quantity of *feces* figured, with some hard lumps. He had made but little complaint since I left him, until about an hour ago, when his cough became more troublesome, and he complained of more pain in the left side of his body. Pulse harder and quicker than in the morning; external heat all over the abdomen, as great or greater than I had almost ever felt it. On examining the blood taken away this morning, I found it as inflammatory as that of the evening of the 31st ultimo. I immediately unloosed the ligature on his arm, and took away about ten ounces more, which produced a sensation of sickness, but neither vomiting nor deliquium. I then administered an injection, and, in less than two minutes, he had a copious evacuation of feces figured, with one or two hard lumps. This relieved him considerably; his body, he said, felt lighter: respiration became easier, and his side less painful, pulse softer, and external heat lessened. I gave him tinct. opii gtt. xxx. in

a dose of the emulsion, and desired that he might have the draught, omitted the night before, towards morning, if he should be very restless and uneasy. Cont. emuls. et elect.

3d, 8 o'clock, A. M. About an hour after I left him last night, he retched very violently, but did not vomit, after which he became easy, and slept well and sound till three o'clock in the morning. Cough and pain in the side abated, and in every respect much better than last night; respiration free; the blood last taken away quite lax and florid, showing not the least signs of inflammation; had had no stool, but made water plentifully. Cont. emuls. et elect. Rep. enema. I desired he might be allowed a little boiled milk during the day, without bread in it; solids totally forbid.

7 o'clock, P. M. Passed a quiet, easy day, having slept the greatest part of it. He became restless, and rather uneasy, about the same time as last evening; more feverish than in the morning, and abdomen more painful; cough rather troublesome; no stool nor vomiting. His bed having now become very uncomfortable, I had him carefully taken out, to have it made afresh. I administered an enema, which staid with him ten minutes, and then came away, without bringing any fecal matter with it. I gave him tinct. opii gtt. xxx. with his emulsion. Soon after taking it, he retched, but did not vomit, after which he thought himself easier.

4th. Mr. A. Robinson visited him this morning, and found him pretty easy; he had passed a good night; no stool nor vomiting. He was ordered for diet today, a little hasty pudding, with treacle, for he now made frequent complaints of hunger. A messenger came in the evening, to inform us, he had been easy all day, but was not quite so well when he left him. Cont. emuls. et elect. et omitt. haust. anod.

5th. Had passed a quiet, easy night, and slept well; free from pain and fever; had a stool yesterday, and one this morning, which was more fluid than the two last, but quite fecal; he coughs sometimes, and expectorates with ease a quantity of thick purulent-like excretion. I removed the dressings from the wound, which were loosened round the edges by the discharge; it looked very well; the sides were not quite approximated betwixt the sutures, the spaces being filled up with



healthy looking granulations; none of the ligatures had separated. I dressed with dry lint and straps of adhesive plaster. He yesterday had taken hasty pudding at three different times, to the quantity of a saucer full in all, which he liked much, and which had agreed with him very well. He had no nausea or vomiting. The same diet to be used today; in greater quantity, if he likes it.

6th and 7th. Going on in every respect very well; had two full evacuations on the 6th. In addition to his hasty pudding, he had been allowed some mutton broth; he also ate a very small piece of mutton. I dressed the wound again today as before, which looked very well: its sides were considerably approximated in every part since the 5th ultimo.

9th. Dressed the wound; one or two ligatures had separated, and, as the rest produced some little ulceration, I took them quite out. In every respect mending daily.

11th. Wound healing rapidly. I allowed him today to get up, at which he was not a little pleased; he soon dressed himself, and walked down stairs, by my taking hold of his arm. Bowels quite open and regular.

20th. Rode on horseback to Ripon, to have the wound dressed, the whole of which was healed, except a very small portion at top and bottom. In every respect very well, and regaining his usual healthy appearance. Appetite and digestion good; cough has left him.

23d. Quite well. The stomach, distended at the upper part of the wound, appears much below its natural situation. I recommended him to wear, for some time, a broad belt, made of stout woollen cloth, round the injured part of the abdomen. He carries himself quite erect, which he did even two or three days after leaving his bed.

#### REMARKS.

It may be matter of surprise and doubt to some, that the *stomach*, as well as the intestines, should have protruded through a wound in the abdomen, situated as described in the case just related; and it may not improbably be conjectured, that some portion of gut had really been mistaken for that viscus.

The *great arch*, with its *appending omentum*, were, how-

ever, too plain and evident to admit of any error of this sort; and my partner, Mr. A. Robinson, on seeing its form externally on the evening of the 31st, distended so low, had no doubt of the fact. Dr. Caley and Mr. Lucas both saw the lad at Ripon, on the 20th, and both saw the stomach distended much below its proper situation: its protrusion, however, is readily accounted for by the manner in which the boy was conveyed home after the accident.

Instead of being laid on a hurdle, and placed upon his back, he was taken before a man on horseback; at this time, but a small portion of gut had protruded, and a handkerchief was given him, with which he was desired to press upon the wounded part, which he, quite overcome with fright and sickness, was unable to do. The horse had proceeded but a little way, before he felt more intestine pushing out, till at length such a quantity had protruded, as by its weight was sufficient to drag out the remainder, and in this way the great arch of the stomach was drawn down. He was carried in that manner two or three hundred yards.

Nov. 20th. Mr. Lucas saw the boy with me today. The stomach still remains much below its natural situation, so much so, as, with the bulging out of the intestines around the wounded part, to give that side of the abdomen a very irregular, prominent appearance.

He has been very well in every respect ever since his recovery; his appetite and digestion are both as good as formerly; and he is capable of enduring labour as well, and to as great a degree as before the accident.

Ripon, 3d December, 1808.

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*Merriman's Case of Suppression of Urine after Parturition.*

From the Edinburgh Medical and Physical Journal, for 1810.

MARY CHAPMAN, twenty-one years of age, a patient of the Westminster General Dispensary, residing in the neighbourhood of Berwick street, Soho, was taken in labour of her first child, on the 13th of October, 1809. Her waters *broke* on the preceding day, but the pains of labour did not come on till



about twelve o'clock at noon, of the 13th. From this time they continued frequent and violent for twelve hours, when the strength of the poor woman, and the patience of her friends being exhausted, they requested that the midwife should send for further assistance, and I was accordingly called up to her, between one and two o'clock in the morning, of the 14th.

When I arrived at the house, the midwife informed me, that the child's head had fallen so low in the pelvis, that the vertex was actually protruding through the *os extremum*, and that she expected it to be born in a few more pains: she acknowledged, however, that the pains were become less efficacious during the last two hours, and that the labour had not advanced since; and she conjectured, with much appearance of reason, that this diminution of uterine action was occasioned by the bladder being full of urine, as the patient had made no water during the whole continuance of the labour. On hearing this report, I laid my hand upon the abdomen, and clearly distinguished by the feel, that the bladder was very full; I concluded, therefore, that there would be nothing more to do than to introduce the catheter, and empty the bladder, after which it was to be expected, that the pains would recover their former strength, and that the delivery would be accomplished without any further difficulty.

The patient being, therefore, placed in a convenient posture, I proceeded to introduce the catheter; in doing which, I found that the child's head was, as the midwife had stated, so low in the pelvis as to render it almost certain that a few pains would expel it; but on passing my finger into the vagina, it (the vagina) felt so excessively hot and burning, as convinced me that the delivery ought not to be trusted to the effects of nature, but that the child must be removed with all proper expedition, or, otherwise, there was great reason to apprehend that mortification and sloughing of the vagina would ensue. It was, however, first necessary to draw off the water; I therefore introduced the catheter as gently as possible, (notwithstanding which, it gave her very great pain, from the inflamed and irritable state of the *meatus urinarius*;) and drew off nearly a quart of very highcolored urine. In a short time afterwards, I introduced the forceps, and delivered the woman of a female

child, which was restored to life with considerable difficulty. In rather less than twenty minutes, the placenta was expelled, and a profuse discharge from the uterus took place at the same time. She had now an opiate given to her, and I left her with very strict injunctions respecting her diet and management.

Before twelve hours had expired, I received information that this patient was suffering great pain from a constant urgency to make water, and an inability of voiding it. I, therefore, again had recourse to the catheter, and emptied the bladder. The orifice of the *meatus urinarius* was swollen and tender, and the passing of the instrument occasioned her much pain; but the sense of heat and burning in the vagina was much diminished.

The usual emollient, aperient, and anodyne remedies were employed to relieve her complaint; but it was necessary to have constant recourse to the catheter for eight or nine days, at the end of which time she began to pass her urine naturally; but she now entirely lost the power of retaining it, and suffered the various inconveniences which accompany an incontinence of urine. I began, therefore, to fear that an ulceration had taken place, and had made a communication between the bladder and vagina; but after a very accurate examination of the parts on two different occasions, I was fully satisfied that no such accident had happened. During the course of ten or twelve days, this continual discharge of urine continued, notwithstanding the use of a variety of remedies that were employed to counteract it; at length I determined to try the effect of the *uva ursi*, and accordingly prescribed a scruple of the powder to be taken three times a day. After a few days, Mrs. Chapman thought she derived benefit from this medicine, and was, therefore, very willing to persevere in its use: It was continued for a fortnight longer, by which time the incontinence of urine was removed, and her health and strength restored.

It has been laid down as a rule in practice, and it is one of those rules, which, being founded in reason and experience, ought not lightly to be deviated from, that the head of the child should be in a situation capable of being delivered by the forceps, for at least *six hours* before they are had recourse



to. As a mean of guarding against a rash and unnecessary use of instruments in the practice of midwifery, this rule is highly commendable; but a rigid observance of it, in the case I have related, would have proved extremely injurious, if not fatal, to the patient: as it is apparent, that a very high degree of inflammation in the vagina, and parts adjacent, was just upon the point of taking place: and had the head of the child been suffered to remain there much longer, so much inflammation must necessarily have come on, as might have proved uncontrollable. It may be said, that when the bladder was evacuated, the pains would have returned; but it is possible, that the return of uterine action might not have taken place for many hours, and in the mean time, incalculable mischief would have been going on; upon the whole, therefore, I am persuaded, that this was one of the cases in which it was proper to break through a useful rule in practice.

A relaxation, or want of contractile power in the *sphincter vesicae*, is sometimes observed after a suppression of urine, arising probably from the frequent introduction of the catheter, and frequent dilatation of the parts; but the incontinence of urine rarely proceeds to so great a degree as in this patient, nor does it last so long. I was led to the use of the *uva ursi* from the celebrity it formerly attained as a remedy in the disorders of the urinary organs in the practice of *de Haen*, and other German physicians. A blister over the sacrum would, perhaps, have been a more speedy remedy, but the constant discharge of urine had produced so much cutaneous inflammation all over the lower parts of the body, as to render the application of a blister unadvisable, at least, in the first instance; and, as the woman was daily mending, under the use of the *uva ursi*, I was afterwards unwilling to discontinue it.

The *uva ursi*, as a remedy in disorders of these organs, is almost entirely fallen into disuse. It has experienced the fate of many once celebrated and fashionable articles of the *materia medica*; having at first been too highly and unjustly extolled, it has now, perhaps, sunk into unmerited oblivion. Lewis, in his "*Experimental History of the Materia Medica*," says of it, "In all cases that have come to my knowledge, it produced great sickness and uneasiness, without any apparent benefit.

though continued for a month." It is probable, that in these cases, the uva ursi was given in too large doses, for I observed no such ill effects in the doses I prescribed to Mrs. Chapman.

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*An account of a case of Lithotomy, with remarks.*

BY THOMPSON FORSTER, ESQ.

Surgeon on the staff of the army, and senior surgeon to Guy's hospital.

From the Medico Chirurgical Transactions, for 1809.

It has been somewhere well observed, that a physician or surgeon of great experience, might write a very useful work, if he would have the courage to give an account only of such methods of cure, and such cases as he had found to be ineffectual, or unsuccessful. It is with this intention, and a firm persuasion of its justness in my mind, that I now venture to communicate the following case and remarks to society.

Henry Brand, a sickly looking boy between seven and eight years of age, was admitted into Guy's hospital, the sixth of August, 1804. I learned from his mother that during the preceding year he had frequently complained of an uneasy sensation, and difficulty in making water, which soon brought on great pain, fulness, and heat over the whole of his belly, attended with thirst, restlessness, and headach; she observed also, that these fits frequently lasted with great violence during two, and sometimes three days.

Some of the patient's symptoms being those of a calculus in the bladder, and his belly being much enlarged, he was ordered, during the first week after his admission, two gentle purgatives, preparatory to my passing the sound. Upon examining the penis for this purpose, I found the prepuce much elongated, thickened, and contracted, so that it was with difficulty I could find the orifice of the urethra, and when this was accomplished, the sound met with impediments from strictures; these also were overcome, and the sound, on entering the bladder, immediately struck against a stone, and by a little



movement of the instrument soon passed by the side of it; but had no range, being precluded from lateral motion by the stone on one side, and the coats of the bladder on the other. A mild purgative, and the warm bath were ordered, to prevent the effects of irritation, which were likely to arise from the preceding difficulties, and in a few days the patient voided his urine much the same as before the examination.

As many of his complaints differed from the common symptoms attending a calculus in the bladder, I determined to defer the operation of lithotomy, till I should have an opportunity of seeing some of those paroxysms before mentioned, which his mother had stated to be of the most distressing kind; I therefore directed a gentle purgative for him twice a week, and soda water for his common drink.

During the fortnight he was under this plan, I observed (even when he was the most free from pain) that his pulse was quick, and feeble, his appetite uncertain, and that a general debility pervaded his whole habit; his urine was sometimes turbid, and at others, deposited a white ropy mucus; this mucus generally succeeded slight pains in his loins.

About this time I had an opportunity of observing the train of symptoms, which his mother seemed to have accurately described; they came on nearly in the following order: anxiety, restlessness, itching and heat along the course of the urethra, more particularly at the neck of the bladder; pain and difficulty in making water; heat, and distention of the abdomen; very quick and small pulse; slight nausea; increased difficulty in voiding his urine, which was soon followed by a complete suppression, so that, for four hours in the succeeding day, neither bougie, nor catheter could be passed; the warm bath and clysters gave relief, but it was four days before he returned to his usual state, and even then it was with increased debility, and a peculiar anxiety, that gave the impression of the child's constantly feeling some internal distress.

About ten days after his recovery from this paroxysm, and when he was more than usually free from irritation, I made particular observation on the quantity of urine he generally voided at one time, which never exceeded an ounce and a half,

and on his power of retaining it at will; and I found that the quantity (even for eleven minutes after the usual excitement to make it had taken place) never exceeded two ounces, from which I was led to conclude, that the capacity of the bladder was considerably diminished, but that it had not lost its voluntary power of retaining the urine, or of propelling it.

I did not propose the operation at this time, from the apprehension of the consequences that I feared might ensue from such great susceptibility to inflammatory action, and from the weak state to which the patient was reduced, and I was also not without a hope, that the powers of medicine might lessen the tendency to inflammation, and restore him to a little better tone and strength to bear such an operation.

In this, however, I was disappointed; it would therefore be tedious and intruding unnecessarily on the time of the society, to go through the detail of medicines prescribed; suffice it to say, that they were found inadequate, either to lessen the violence of his fits, or to augment his strength, for in the month of October he had a more than usually violent attack, that lasted him five days, and reduced him to the lowest ebb.

It was a few days after this last violent attack, that I first perceived his urine dribbled from him involuntarily, that he had a constant sense of smarting in the whole course of the urethra, and that he had lost all power of propelling the urine forward, or of retaining it. Even the action of the abdominal muscles, as in straining, produced no water from the bladder.

Here a new symptom presented itself to our notice, which is worthy of our most serious attention, namely, that of an inability in the bladder to act upon its contents, arising probably from the repeated inflammatory actions to which it had been subjected; yet the general irritability of the system remained as great as ever, and it seemed even to increase with his diminished strength.

The patient being in this wretched state, a consultation was held on the second of November, when a retrospect of his past symptoms, and a careful examination of his present ones were fully considered, and it was determined that the operation should be immediately performed, though it was perfectly



understood to be under the pressure of the most disadvantageous symptoms; yet it was thought to hold out some faint hope of relief; and it seemed certain, that, unless it was had recourse to, he must inevitably sink, worn out by constant pain and misery.

I performed the operation next day. The obstructions in the urethra before mentioned, being with some difficulty overcome by the staff, an impediment to the full introduction of the gorget occurred, that certainly arose from the beak of the instrument coming directly against the stone, which prevented its farther introduction into the bladder, till it evidently slipped between the stone, and the coats of the bladder, where it was tightly wedged in; however as I could feel the stone with my finger at the end of the gorget, I passed the forceps, and, with considerable difficulty, expanded them sufficiently to lay hold of the stone, which broke with moderate compression. About two-thirds of it came away in the forceps, preserving so evidently its convexity, as clearly to give the form, and an idea of the quantity remaining in the bladder. This being but small, was soon removed, and the patient taken to his bed.

Symptoms of inflammation soon manifested themselves, and extended to the abdomen, loins, and thorax. The warm bath, poppy fomentations, leeches, clysters, &c. were carefully, though ineffectually, administered, as the patient died on the fourth day after the operation.

On examining the body immediately after death, the belly was observed to be of a dusky hue, emphysematous, and distended. Not getting permission at that time for farther investigation, I was obliged to wait ten days before I could give the appearances of the interior parts, which were as follows:—On dividing the integuments, the same dusky color was noticed, which is a morbid tint, well known to those who are conversant in inspecting diseased parts, and perfectly distinct from that which is the result of putrefaction after death.

As nothing peculiar presented itself in the viscera in general, I shall confine my account to the state of the kidneys, bladder, and rectum. The left kidney was very small, loose in its texture, and its ureter scarcely discernible; the right kidney

was enlarged and flabby, its ureter about the size of a goose quill, and much incrustated with calcareous matter, where it entered the bladder. The bladder was very much diminished; hard, and resisting to the touch. On the left side was discovered the wound made in it by the introduction of the gorget; and a section being made through the bladder on the right side, and a portion of it removed, brought to view the great thickness of its coats, its lessened cavity, and a considerable deposit of calcareous matter adhering to the inner surface. The rectum was nearly as usual, except only, that it participated in the inflamed and discolored appearance of all the neighbouring parts.

From the foregoing history of this case, and from studying the morbid appearances, some useful observations are deducible, that may tend to strengthen the judgment of the young practitioner, and assist him in forming his opinion, and in reasoning from the symptoms up to their causes.

To elucidate this more clearly, it may not be improper to make a short recapitulation of some of the leading symptoms. In the first place, the unusual susceptibility to inflammatory action, that showed itself so often over the whole belly, is a symptom not very frequent, and when it does occur, is rarely overcome; moreover it appears from this case, that each succeeding inflammatory attack made such havoc in the constitution, as to bring on worse symptoms, and increased debility. Therefore, when so alarming a symptom has clearly manifested itself, with all its train of consequences, and when the primary cause has decidedly been found to be a stone in the bladder, I should be inclined to allow but a short time for a trial of the powers of medicine to strengthen the habit and to allay such irritation, but should propose the operation at an early period, while the bladder retained its power of acting on its contents. I am aware that this would be to perform the operation under very unfavourable circumstances, yet the case before us sufficiently shows, that there is no reason to expect any amendment can take place, whilst the original cause of the mischief remains; and it may also be observed, that in such instances, each succeeding fit renders the bladder more likely



to be deprived of its muscular powers; and when such an event has actually taken place, great doubts may arise as to the propriety of venturing on the operation at all. For if the immediate cause of this privation of action should proceed either from local paralysis, or from the deposit of coagulable lymph, great impediments in either case will probably arise to obstruct the progress of healing, if not to subvert its first principles; should it arise from the former cause, the want of nervous energy might leave the wound in statu quo, till nature were quite exhausted; and if from the latter, (that is from the deposit of coagulable lymph) the muscular fibres of the bladder might be completely blocked up by such an accumulation, and the process of healing would of course be greatly impeded by this mechanical pressure, so that, though the first principles of the healing process might not be destroyed, they would be at least so far weakened as to leave great doubts of their possessing sufficient power to shoot forth the new substance necessary to complete the healing of the parts.

From this view of the case, it would seem, that, after the inability of the bladder to act upon its contents has shown itself, a very different practice must follow, from that where the bladder possesses its powers of expulsion, and retention; in cases where these remain, I have recommended proceeding early to the operation for the reasons I have given, but in cases where the bladder has lost those powers, I do not think the operation advisable, or that it would be attended with success.

I must acknowledge that my reflections on this case have influenced my practice, and I have since cut, at an early period, one patient, in whom the extreme tendency to extensive inflammatory action showed itself, nearly in as great a degree, as in the instance I have related; the inflammation from the operation was considerable, but it was moderated by the usual remedies in a few days, and in five weeks the patient was perfectly well.

Since that time I have refused to cut two patients, where the debility was considerable, and where the action of the bladder was entirely lost.

I have thus given to the society a narration of the symptoms, progress, and fatal termination of the case in question, and of the appearances on dissection; and I am not without hopes, that the information to be derived from it may have some beneficial effect in the future treatment of similar cases. The object I have had in view, by this communication, will then be completely attained.

T. FORSTER.

Southampton Street, Bloomsbury-sq. Jan. 7, 1806.

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*An account of three cases of sudden death, with the appearances on dissection, and some additional observations.*

BY THOMAS CHEVALIER, ESQ. F. R. S.

Surgeon to the Westminster general dispensary, and surgeon extraordinary to the prince of Wales.

From the Medico Chirurgical Transactions, for 1809.

AT a former meeting of the society I related the cases of three persons who had died suddenly, and whose bodies I had been called on to examine; and as the immediate cause of their death appears to be one not generally noticed by anatomists, I have great pleasure in complying with the request of the society by detailing the circumstances again in this paper.

The first case occurred in September, 1806, in Mrs. C. a young lady at Greenwich, who while talking with her husband complained of being faint, and desired to be laid down. She was led to the bed, and was supposed to fall asleep, as he still continued to support her. In about twenty minutes he rang for the servant, who, on entering the room, exclaimed—"My mistress is dead!" So it unhappily proved; and I was desired on the following day to investigate the cause of this most affecting event.

The head was first opened, and the brain carefully dissected; but not the smallest vestige of disease could be perceived in any part of it.



On examining the trunk of the body, no morbid appearance presented itself in any of the viscera. The uterus was in a state of impregnation, so early, that the fissure in the ovarium, from which the ovum had escaped, was yet distinctly to be seen.

This apparent absence of any disease, led me to pay more particular attention to the state of the heart; on opening which, I was much struck with its extreme flaccidity, and the entire emptiness of all its cavities, in neither of which was the smallest quantity of blood. There was blood however both in the vena cava, and in the pulmonary veins.

I had never, on any former occasion, seen the heart so completely empty. It is not unusual to find the ventricles shut up by muscular contraction, so as nearly to exclude blood from them; but here both auricles and ventricles were destitute of it, without either of them being in a contracted state.

It therefore appeared to me that this lady had died from a syncope, or rather, asphyxia, of a peculiar kind; in which the action of the heart had ceased for want of the regular supply of blood from the returning vessels.

A few weeks after this occurrence, I was again called to Greenwich, to examine the body of an elderly man, who had recently recovered from a maniacal affection, and who, after falling suddenly from his chair, breathed short for a few minutes, and then expired.

I went to this dissection fully prepossessed with a persuasion that he had died of apoplexy; and I was much surprised, when, on the most careful examination of the brain, I could find no morbid appearance, except an ossification of the falx, which must have been of long duration, and though it might in some degree account for his mania, could not at all account for his death.

I therefore proceeded to examine the body; for immediately the circumstances of the former case occurred to my mind, and also to Mr. Harrison, who had been called to both patients, and was present at the dissections.

The state of the heart was exactly similar. All the cavities

were empty, but uncontracted; and the vena cava was also empty to the distance of several inches from the auricle. No other appearance could be detected in any viscus, by which death could at all be accounted for.

The last case occurred lately in a lady who died shortly after having been delivered of twins. I was informed she had for some time entertained apprehensions that she should not survive her delivery. After the birth of the second child she appeared a good deal exhausted, and as the discharge of blood was very moderate, the accoucheur thought it best to defer the extraction of the placenta. She recovered a little; but about two hours afterward grew suddenly faint, breathed short, and died in about half an hour.

In this case I did not examine the head, nothing having existed to excite a suspicion of mischief there. All the viscera were free from disease. The uterus contained the placenta with a small quantity of blood. But all the cavities of the heart were in a state of relaxation and completely destitute of blood. There was no blood in the vena cava near the heart, and the emptiness of its ascending branch extended as low as the iliac veins.

In all these cases the heart itself, and its valves, were free from any disease or alteration of structure.

I have hitherto been able to find only two similar instances recorded by medical writers. One of these is mentioned by Bonetus, Sep. Anat. vol. i. p. 883. A woman, more than forty years of age, complained suddenly of dimness, noise in the ears, and headach. She soon after lost her voice and pulse, and died in four hours from the attack. She had previously laboured under pectoral complaints, and on opening the body, the lungs were found coated with coagulable lymph, and abounding with pus in their substance. But the most remarkable circumstance, and which alone explained the manner of her death, was, that neither in the heart, nor the adjoining vessels, was a drop of blood to be found. "*Neque cor, neque vasa adsita, vel guttam sanguinis continebant.*"

The other case is recorded by Morgagni, Epist. 48. Art. 44. and very much corresponds with the third of those I have related above. It was of a woman, who, during her pregnancy,



had some presentiment that she should not survive her labour, although she had already been the mother of several children. She was delivered of a daughter; but the placenta did not come away. An hour afterward, she was suddenly seized with dejection of spirits, coldness, and loss of pulse, and died in an hour and an half from the attack. On dissection the heart was exceedingly flaccid; scarcely any blood was found in the auricles or right ventricle, and none at all in the left. "*Cor supra quod dici possit flaccidum, nihil fere sanguinis in auriculis, dexteroque ventriculo, nihil autem prorsus in sinistro continebat.*" The state of the adjoining bloodvessels is not noticed.

The disease I have now described may perhaps be properly termed *asphyxia idiopathica*. The essential circumstances of it evidently denote a sudden loss of power in the vessels, and chiefly in the minuter ones, to propel the blood they have received from the heart. In consequence of which, this organ, after having contracted so as to empty itself, and then dilated again, continues relaxed for want of the return of its accustomed stimulus, and dies in that dilated state.

But it is remarkable that death was not produced instantaneously in either of these cases. So that it is probable the larger veins continued their office a little while after the attack; and that blood flowed from them for a short time, into the heart; feebly indeed, and in small quantity, but still enough to keep up a little action in it. It is also probable that the debilitated state of the extreme vessels, though sudden and great, was not complete at first; especially in the last of the three cases I have related, and in those quoted from Bonetus and Morgagni: but the action of the heart gradually failing for want of supply, that of the larger arteries would soon fail also, for the same reason, and death must then follow of course.

The flaccid state of the heart is probably produced by its own vessels partaking of the general debility, and thus inducing an atonic state of its muscular fibres.

The weakened state of the circulation through the pulmonary vessels will account for the short, or to speak more correctly, the feeble breathing, which commences with the attack, and continues till its termination.

Although I feel quite unable to account satisfactorily for the first production of such a disease, it nevertheless appears to me that these histories lead to some important practical conclusions. Such as have occurred to myself I shall take the liberty to point out. Others may probably be supplied hereafter by practitioners, who, aware of the nature of the disease, and called in immediately on its commencement, will direct their attention to its various circumstances, more particularly than may hitherto have been done.

The impropriety of taking away blood in such a case, is, I think sufficiently evident; nor indeed does it seem likely that an attentive practitioner would confound it with any in which that evacuation is called for. The state of the pulse, and particularly of the respiration, would be a sufficient distinction, and clearly point out the necessity of the most prompt and active measures to stimulate the extreme vessels, to accelerate the returning blood, and to excite the nervous energy.

It would therefore be proper to lay the patient as speedily as possible in the horizontal posture, and to give internally some highly stimulating liquid; as brandy, wine, ether, volatile alkali, &c. and these should be given in a tepid state, lest their coldness, when first swallowed, should retard their salutary influence on the extreme vessels and nerves of the stomach. Spices may also be employed, and in this, as in many other cases, would probably be borne in larger quantities than are commonly exhibited. A scruple of the *Pulvis Aromaticus*, for example, may be given for a dose. Brisk friction of the surface of the body, and particularly of the extremities, should be employed, and the patient covered with warm blankets, taking care however to leave the face open, and on no account to impede the freest possible access of pure air to the lungs. A hot bath might be of use in some instances; and it would probably be more effectual if it were prepared with salt water, or strongly impregnated with some stimulating ingredient, as capsicum, pepper, ginger, &c. But some discretion is certainly requisite in the employment of a hot bath, where great debility prevails, lest the exertion and fatigue attending its use, should increase the irritability in too great a proportion, and exhaust the remaining strength.



A pint or more of camphorated mixture thrown up warm, as an enema, by means of a syringe, might also be of use. Some ether, or tincture of castor, or assafœtida, might perhaps be added with advantage. But in this, as well as in the employment of the warm bath, care must be taken not to over fatigue the patient; for no debility will be found more insuperable on many occasions, than that arising from fatigue. So that these remedies will perhaps be most useful after something stimulating has been given by the mouth, and is beginning to produce its effect.

Other means may be suggested by the particular circumstances of different cases; and where the nature of the attack is known, one cannot but hope it may in some instances be remedied. It is with this view I have brought the subject before the society, and I shall feel highly gratified if these imperfect observations should be the means of bringing forward more complete information respecting it, than I have been able to impart.

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Some time after I had delivered to the society an account of the above cases, I was conversing on the subject with my friend Mr. Charles Woodd, of Edgeware road, who informed me that he had recently met with two cases which appeared to him very extraordinary, and which he could not help considering to be of a similar nature to those I had mentioned. When he related them to me, I was entirely of that opinion, and requested he would favour me with an account of them in writing, that I might lay them before the society. This he has obligingly done, and as both cases terminated favourably, I have no doubt the society will consider them as a valuable addition to the paper already alluded to.

"CASE I.—At six in the morning of August 28, 1808, I was desired to visit Mr. A. who had been attacked about one o'clock, with an uneasy sensation in the thorax, difficulty of respiration, and a sense of extreme lassitude. I found him with a pulse hardly perceptible, and not more than twenty-nine in a minute, although the vessels of the skin, and tunica conjunctiva were loaded with blood. Previous to my being sent

for, he had taken three large spoonfuls of Reymer's tincture (a very stimulating preparation), and one bottle of a mixture which he generally kept by him, having been subject to spasms, and which contained mist. camph. ʒvj. sp. lav. c. ʒss. sp. ammon. c. ʒijj. The anxiety still increasing, he took three tea spoonfuls of what appeared to be camphorated liniment, and some brandy. Notwithstanding all this, the action of the heart had decreased. When I saw him, the sense of fainting and difficulty of breathing became almost insupportable. I immediately gave him a tea spoonful of ether vitr. and repeated it every ten minutes, till he had taken an ounce, but without the least effect, either on the pulse or his general feelings. I therefore continued to give more stimulants; and by eleven o'clock he had taken, in various preparations, in addition to what has been before mentioned, ammon. ppr. ʒss. t. opii gtt. 40. tinct. castor. ʒss. ten drops of the medicine called the black-drop; and two drachms of the sp. ammon. comp. with camphor mixture. At twelve, an enema, with sixty drops of tinct. opii, was administered, and two drachms more of ether had been taken. Friction had been used along the spine with ether and volatile spirit, and also over the sternum. The pulse now became more perceptible, and gradually increased in strength till two o'clock, when he went to sleep, but with the breathing still laborious. At four o'clock he awoke; a blister was applied to the sternum, and he took a draught every four hours, composed of gutt. nigr. gtt. v. aq. ammon. acet. ʒij. mist. camph. ʒj. sal. c. cerv. gr. iv.

"On the following day he felt nearly recovered; nor did all this quantity of stimulating medicine produce the slightest degree of fever. He has ever since continued in health."

"CASE II, was much slighter. Mr. W. after a long walk, was suddenly seized with great difficulty of breathing and faintness, so as to be unable to stand, or speak distinctly. He was immediately taken home, and I found his face suffused with blood, his breathing difficult, with great anxiety, and his pulse scarcely perceptible. He imagined himself dying. I gave him immediately two drachms of sp. ammon. c. and in ten minutes one drachm of ether, and thirty drops of tincture of opium. In about twenty minutes he was relieved. A blister was how-



ever, applied to the chest, and in the course of the evening he took a mixture composed of conf. opiat. ʒj. ammon. ppr. ʒss. aq. cinnam. ʒiij. The next morning he was much better, and has ever since continued well."

The latter of these two cases may at first appear only an attack of common asphyxia; but its affinity to the rest is, I think, distinctly marked, by the suffused appearance of the countenance; which evidently showed the detention of the blood in the extreme vessels. The former case is however, more remarkable; and its favourable termination seems to be principally owing to Mr. Woodd's judicious and persevering exertions.

December 13, 1808.

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*Experiments relative to the Influence which the Nerves of the Lungs exercise on Respiration.*

Read before the National Institute, by M. Dupuytren, Chief of the Anatomical Department in the Medical School of Paris, Assistant Surgeon to the Hotel Dieu, &c. &c.

Translated for the Eclectic Repertory.

THE memoir which I have the honour to present to the institute, contains the history of some experiments made with the view of determining the kind and degree of influence of the vital action of the lungs, in the changes which the venous blood undergoes in passing through these organs.

For some years, two opinions have prevailed with respect to the primary cause of respiration in animals. According to one opinion life is indispensable to the exercise of respiration as it is to all the other vital functions: the air might be even taken into the chest, and come in contact with the blood without producing any manner of change if the lungs were not endowed with life.

Agreeably to the other opinion, when the air has pervaded the lungs, and the venous blood has passed through the pulmonary vessels, the action of chemical affinities is sufficient for respiration to take effect through the very delicate membranes which separate these two fluids.

This second opinion, supported by some experiments has been adopted by all those who would rather rest their ideas on

facts, however insufficient, than upon assertions totally devoid of proof.

In this state of the science the advocates of the vital causes, and the partisans of the chemical causes, as applied to the explanation of the phenomena of life, will consider the question which I propose to examine, as already solved according to their respective principles, but it has not been by direct fact. Not but that experiments analogous to those which I am going to relate have been tried; but as almost all of them have been made with intentions very different from ours, it does not appear that they ever imagined that result which we have obtained.

Thus Haighton, Cruikshank, Meyer and others, performed the division of the eighth pair of nerves, which furnish those going to the lungs, with the view only of observing the regeneration of the nerves; and any one may be convinced on perusing the history of their experiments, that they did not even inquire into the cause of the death of those animals in whom they had divided at the same time the two nerves of the eighth pair.

Bichat is the only author I am acquainted with, who has attended to the influence exercised by the brain, by means of the eighth pair of nerves, in respiration, and I must not conceal, that he has concluded from his experiments that this organ has no direct influence on this function. But I must also observe, that as his principal object was to determine if it were by the cessation of the action of the muscles of the chest, or by that of the lungs, that death takes place after injuries of the brain; he could not fail to discover, what is the fact, that death occurs more readily in this case, from the interruption of the motions of the chest, than from the ceasing of life in the lungs. If Bichat had not confined himself to this first conclusion, he would have recognised a second means of the brain's influence on the lungs, that which I shall endeavour to demonstrate.

Our object being to determine the effects of life on the changes which the blood undergoes in the lungs, it would be important to discover the means of destroying, or at least of completely suspending it in these organs; from whence it



would be easy to infer the extent to which life is necessary to the exercise of respiration; but as this destruction or suspension could not be accomplished without destroying the life of the animal, we were obliged to recur to other means.

It is well known that in the higher order of animals, life results in all the parts from a certain number of actions, for example, of those of the nerves, arteries, &c. and that it cannot long exist when any of these are destroyed. But it is evident, that if by destroying one of these conditions by which life exerts itself in the lungs, respiration should cease, we could not hesitate in attributing to the intire life, a power equal at least to one of these conditions. In pursuing this idea, we considered the nerves of the eighth pair which furnish those of the lungs, as being the organs by which the life of the lungs might be modified without the neighbouring parts being in the least affected.

It is true where the change in the pulmonary nerves might not have induced a corresponding one in respiration, nothing could be inferred from these experiments against the influence of life on this function; it might still be imagined that a greater impression made on the life of the lungs, would have stopped respiration; but if the latter should be considerably affected, or entirely suspended by the mere lesion of the pulmonary nerves, the conclusions from our experiments in favour of the influence of life on respiration, would be correct, and this is the fact as we shall presently show.

Before we enter on the detail of these experiments, we beg leave to premise some well known facts respecting respiration. This function consists in the intimate and reciprocal action of the air on the blood of animals, for the purpose of rendering this latter fluid proper for exciting and nourishing the body. In man and in many animals this function is accomplished by means of two distinct organs; one muscular, and intended for alternately drawing the air into the chest, and again expelling it; the other furnished with cells and vessels for receiving the external air, and transmitting the blood returning from all parts of the body. Respiration takes place only in the lungs, the other recited organs are only means employed by nature for facilitating this operation.

The lungs have not as some organs, a separate pair of nerves. Those which they receive are chiefly derived from the eighth pair. These two nerves arise from the sides and beginning of the medulla oblongata; immediately upon leaving the skull they give several branches to the pharynx; they run down the front and sides of the neck, and furnish in their route the superior laryngeal nerves, and some filaments destined to the heart. They then penetrate the chest, where they at first furnish some branches which uniting with branches from the great sympathetic form the anterior pulmonary plexus. They afterwards give off the inferior laryngeal nerves, and soon after numerous filaments, the reunion of which forms the posterior pulmonary plexus, whence is derived the greater part of the nerves of the lungs.

The pulmonary nerves are not distributed to the whole extent of the lungs; but according to the observation made by Scarpa, in the explanation of the fifth Table in his excellent work on the nerves of the heart and lungs, they accompany the bronchia and their divisions, to which they are closely applied, and seem much less intended for the parenchyma of the lungs, than for the sides of the air tubes and the membrane which lines them.

After giving rise to this plexus, the nerves of the eighth pair descend along the œsophagus, enter the abdomen, and are distributed to the stomach, liver, gallbladder, spleen, duodenum, and many other parts through the medium of the solar plexus.

The lungs receive, beside the branches sent from the eighth pair, other nerves, which arise from the superior cervical ganglions, the median and inferior ganglions of the great sympathetic, and from its first thoracic ganglion. These nerves, in some degree supernumerary, serve to explain one remarkable circumstance in our experiments.

We commenced these experiments by laying bare in a horse the nerve of the eighth pair, by an incision made on one side of the neck; and after separating this nerve from the great sympathetic to which it is only bound by loose cellular texture, we divided it without the animal expressing much pain, and without the respiration being sensibly affected. We observed



only a slight disorder in the circulation, his lips were inclined to a violet color, but when freed from his trammels he rose, walked, ate, and drank as usual; in short, he suffered no serious inconvenience from the division of the nerve of the eighth pair, and the wound in his neck healed in a short time.

Some days after having performed the division of the nerve of the eighth pair, on one side, we threw down a Hungarian horse, a dapple gray, aged between seven and eight years, of tolerable strength though glandered. We began by dividing one of the nerves of the eighth pair, and he gave no greater signs of pain or alteration in the pulmonary functions, than we had observed in the preceding experiment; but no sooner did we seize the other nerve of the eighth pair, in order to divide it, than the horse was thrown into the most extraordinary agitations, and no sooner was the nerve cut, than opening his mouth very wide, dilating his nostrils, extending his neck, exerting all the powers of inspiration, the animal breathed very laboriously through his mouth and nostrils, and uttered a horrible plaintive sound, like a brokenwinded horse, but much louder and more frightful. At the same time he made violent and vain attempts to vomit. His lips, tongue, and inside of his mouth acquired a red violet, and then a livid color.

He soon rose, but already weakened, and agitated with violent twitchings, having no certain gait, he lay down, stretched himself, rolled several times on the ground, and at length expired at the end of an hour and a half, in the most dreadful agonies, such as no pen can describe. On opening his body we found that the eighth pair of nerves had been completely divided, and that one of the great sympathetics had been slightly injured.

After being convinced, by repeating this experiment many times, that it was always attended with the same results, some slighter shades of difference excepted, relative to the intensity and duration of the symptoms, which depended sometimes on the strength of the animal, at others on the more or less complete division of the nerves of the eighth pair, and on the lesion of the great sympathetics, we inquired into the immediate cause of these dreadful phenomena. We supposed

that it might depend on the interruption of the hæmatosis in the lungs. In order to decide this question, we threw down a stallion of a dirty black color, aged fifteen or sixteen years, having the glanders, and being spavined in one leg. We laid bare successively the two nerves of the eighth pair, and the facial artery on one side; we then opened the artery, and after being satisfied, that the blood which spirted from it was of a vermilion color, we divided nearly at the same time, the two nerves of the eighth pair. Immediately we observed the same symptoms as we have mentioned above. The blood which now flowed from the opened artery, appeared black, and not red as it was before. In a short time the color acquired such intensity, that it was difficult to conceive how life could be maintained by blood, which so strikingly resembled venous blood. After having verified so remarkable a change, by the repeated examination of the blood furnished by the punctured artery, and by the comparison of several portions of this blood, and that of a sound horse, we were desirous of examining the state of the blood contained in the veins, and to our surprise we found it blacker than that in the arteries.

We have since repeated this experiment many times, and have always observed the same effects whenever the experiment has been exactly repeated. Without entering into a detail of each particular history, we shall now present the general result of the experiments we have made.

The horses in whom only one nerve of the eighth pair has been divided, experienced only a temporary inconvenience, their blood scarcely changes its color, and they soon get well.

Those in whom the nerve of the eighth pair has been entirely divided on one side, and to the extent of one half or two thirds on the other, at first labour under all the symptoms which follow the complete division of the two nerves, but these symptoms abate at the end of some hours; and if the animals are not worried, or made to run, they recover, and it is astonishing to see that the sixth, or even a smaller part of the nerves of the eighth pair is sufficient to maintain respiration; but whenever we force these horses into a gallop for a few minutes only, all the symptoms arising from the division of the nerves immediately occur, and sometimes terminate in



the death of the animal submitted to the experiment. We more particularly observed this in a horse in whom we had not left more than the seventh part of the eighth pair of nerves; a month after the experiment he appeared in good health, but when we made him run, he had hardly gone two hundred meters, than he began to blow, breathing through his mouth and nostrils, he lay down, rolled on the ground; his body was covered with sweat, his blood became black, and it was not till the end of half an hour, that these symptoms disappeared. And since then they have been renewed whenever he has been made to run.

The simultaneous division of the two nerves in the horse occasions the most alarming symptoms, and proves mortal in a space of time, never less than half an hour, and never exceeding ten hours.

The most usual phenomena of this latter division, are a violent agitation of all the limbs; efforts to vomit, always ineffectual in the horse; the opening of the mouth; the dilatation of the nostrils; a very loud plaintive sound, expressive of great pain and anguish; the black color of the arterial blood, that of the veins still darker.

In making these experiments, we did not confine ourselves to horses; we have repeated them on dogs; the general results they presented, are as follows: It will not be improper to remark, that in these animals it is extremely difficult to separate the great sympathetic nerve from the eighth pair, because the latter forms a groove, from which it must be disengaged, if we wish to avoid dividing it with the nerves of the eighth pair.

When we have succeeded in separating these nerves, and have divided that of the eighth pair on one side, the animal seems scarcely to suffer any effects from the experiment. But if we divide this nerve on both sides at once, the dog vomits, loses his voice, he opens his mouth very wide, the linings of this cavity assume a violet color; he becomes weak, lies down, crawls, or rather rolls on the ground, as if his strength were exhausted. The respiratory motions continue, excited however, by an illusive sensation; he extends his neck and muzzle, as if to catch the air. The color of his mouth becomes more and more livid, the blood in the arteries turns

black, that in the veins looks as if it had been charred. At length, faint and exhausted, he dies at the end of one, two, or three days at farthest.

From the preceding observations, it is evident, that the division of the nerves of the eighth pair, produces the same effects in dogs as in horses; it must be acknowledged that these effects are not so great in dogs, which may depend on their having less occasion to breathe than horses, or on their lungs receiving more nerves from the great sympathetic.

The constant result of these experiments is that the simultaneous division of both nerves of the eighth pair, produces death after some hours in particular animals, and after some days only in others, and that it prevents the hæmatosis in the lungs.

In order to establish the latter consequence, it remained to determine, whether there exist no other causes of death, and of the phenomena which accompany it, than the impaired state of respiration.

It did not seem probable that the pain and wounds occasioned by the division of the nerves of the eighth pair, could produce the phenomena we have described. In fact, the wound and pain resulting from the division of one of the nerves were so trifling that it was difficult to conceive how the division of the two nerves could produce effects so widely different.

However, to remove all possible doubt, and to subject the result of the preceding experiments to a decisive proof, we resolved to make compression on the eighth pair of nerves, strong enough to suspend their action, but not to destroy their organization. It is well known that the compression of the nerves produces the same effect as their entire division, with this difference, that they recover the power of acting when they have been only compressed, but that they irrecoverably lose it when they have been divided.

It is evident that if the phenomena which we have observed depend solely on the interruption of the action of the eighth pair of nerves on the lungs, that they will take place on compressing these nerves, and again disappear on the power of acting being restored to these parts.



We chose for this decisive experiment, a middle sized dog, and after laying bare the carotid artery, and satisfying ourselves that the arterial blood was of a vermilion color, we passed a ligature round the nerves of the eighth pair, and then made the requisite degree of pressure *proposed*. It had scarce been made, when retchings and violent fits of vomiting came on; the lips, tongue, and the inside of the mouth acquired a violet color, the arterial blood also became in a short time of a deep black color; yet there had been no interruption in the breathing. At the end, however, of some minutes, the animal weakened and gasping for breath ceased to move the chest. He seemed even for some minutes to be completely deprived of life, but on removing the compression from the nerves on the first signs of this apparent death, we soon observed some motions of inspiration and expiration, at first feeble and irregular, but which acquired force and regularity: and when perfectly reestablished, the livid and violet color of the eyes, lips, tongue, and inside of the mouth disappeared by degrees. From that time, the blood which was allowed to escape from the artery insensibly lost its black color, and in a short time became vermilion. By compression alternately made and suspended with nice attention, we could cause the same dog to pass several times from life to apparent death. These experiments, when carefully conducted, are attended with so little danger, that many animals submitted to them have perfectly recovered; and one dog on whom they were often repeated, we have kept for some time. The effect of this compression may be compared to the asphyxia produced by a non-respirable, but not deleterious gas, which interrupts respiration for a time, but occasioning no serious detriment to life, does not prevent respiration from being again restored and perfectly established.

Before we close what we have to say respecting the compression of the eighth pair of nerves, we will remark that its effects are always more immediate and dangerous than those from the division of the nerves, and we have had occasion to make the same observation on other nerves.

However conclusive the experiment we have related must appear, yet as it was performed on a part of the nerve of the

eighth pair in the neck, above that point whence are furnished very large branches to other organs besides the lungs, doubts might be entertained whether the phenomena observed were simply the result of the injury done to the life of the lungs, or whether they were not owing to the disturbance manifested at the same time in all the organs to which this nerve is distributed from below this point.

To remove all doubts it became necessary to divide separately the branches of the eighth pair sent to the lungs, and to allow the rest of the nerve and its distributions to remain; or if this were impossible, to divide successively all the branches sent to the other organs, and to allow the trunk and the branches sent to the lungs to remain untouched.

We adopted the latter as being the only one which could be employed, and as being as completely in point as the former; for it is evident that if, by successively dividing all the principal branches of the eighth pair of nerves, those going to the lungs excepted, none of the above described phenomena should be produced, there can be no hesitation in considering the cessation of the action of the eighth pair of nerves on respiration as their true and only cause.

We began by dividing the superior laryngeal nerves in a dog; the only consequence was a weakness of voice attended with disagreeable hoarseness, which subsided after a time. We should have been desirous of dividing the filaments going to the heart with the view of determining the effect on the motions of this organ, and of their consequent derangement on life; but it is well known that it is almost impossible to perform this experiment completely.

We then divided the two superior laryngeal, and the inferior laryngeal recurrent nerves in another dog; the voice was completely destroyed, as has been observed by many physiologists since the time of Galen. None of the symptoms of asphyxia produced by the intire division of the eighth pair of nerves occurred, either during the operation, or for a length of time which the animals lived, who had been subjected to experiments fraught with so little danger.

It now remained that we should divide the nerves arising below the pulmonary plexus with the view of closing this kind



of analysis, and of assigning to each branch of the eighth pair, the share it had in the phenomena exhibited by our experiments. This very difficult division could only be performed by opening the left side of the chest in a living dog, and by searching with the fingers for the stomachic cord twisted upon the œsophagus, the animal still breathing with the right side. Having discovered these nerves it was easy to divide them with a pair of scissors; at that instant the most dreadful retchings, and violent vomitings came on, and it often happened that we could neither restrain them, nor retain the lungs within the chest, and the animals died in our hands. We were, however, fortunate enough to preserve the life of many of them for some days, by closing the wound in the chest with a suture. Yet in this case we observed no signs of asphyxia, nor of change in the color of the arterial blood to black.

From these experiments it appears that the influence of the branches of the eighth pair of nerves, is confined to the parts to which they are respectively distributed; that the division of the superior laryngeal nerves, inferior laryngeal, and the stomachic cord produce only hoarseness, loss of voice, nausea, and vomiting, and that we must refer the production of the symptoms of asphyxia to the pulmonary nerves alone.

This being established, it remains that we should obviate some few objections, so as to give the greatest possible weight to the conclusions presented in this memoir.

May not the asphyxia which we have just announced depend on the disorder which usually arises in the motions of respiration, after the division of the eighth pair of nerves? But this disorder, however remarkable it may be, is no more than what sometimes occurs on other occasions which, nevertheless, are not followed by asphyxia or death. Besides, this disorder in the motions of respiration is not inseparable from our experiments, and we have more than once seen the hæmatisis when these motions had undergone no sensible alteration.

The nerves are essential to the nutrition of the organs, and to the exercise of their functions: whence it results that by destroying the integrity of those nerves, the nutrition and functions of these organs must be at the same time affected.

But is it by solely suspending the nutrition of the lungs that the asphyxia is produced? That is scarcely probable; for nutrition is, in its very nature, a slow operation, and cannot be stopped in an instant, and when it ceases, does not produce instantaneous effects. Nevertheless, convinced that we ought always to prefer experiment to reasoning, we divided one nerve of the eighth pair in one dog; and in another, we intirely cut across one nerve of the eighth pair, and the other to one half; we performed the same experiments on a horse, and we allowed these animals to live for more than a month, yet we never discovered, on opening their bodies, any marks of atrophy, wasting, or alteration in their lungs.

This fact, in conjunction with some others, particularly with the observation from Scarpa, that the pulmonary filaments from the eighth pair, are distributed almost exclusively to the bronchiæ, and their internal membrane, would seem to indicate that their functions are especially connected with respiration.

There is one other objection, which appears much stronger than any of the rest, and which must not be passed unnoticed. If respiration is under the influence of the eighth pair of nerves, as our experiments seem to demonstrate, how happens it that death is much longer before it takes place in this kind of asphyxia, than in those produced by the want of respirable air? In the first place, it may be remarked, that in a divided nerve, that part which is separated from the brain still retains the power of determining the motions and actions. Again, the filaments derived from the great sympathetic through the medium of the pulmonary plexus, may preserve respiration for some time after the destruction of the eighth pair. One circumstance which we have more than once observed, in our experiments, gives great weight to this last opinion; it is that horses, in whom the great sympathetic was divided at the same time with the eighth pair, have always perished much sooner, than those in whom the latter were alone divided. Hence it is probable that the filaments which the great sympathetic sends to the lungs, may preserve in them a power capable of retarding for some time the change of the blood to a black color. And, observations made on men having defects in the organization of the heart, which permit the black blood



to mix and to circulate with the red, show that an animal may live for a long time with a mixture of these two fluids, in very different proportions. They likewise show that these persons remain habitually in a state of muscular debility and oppression of strength, very analogous to what we have observed in brutes.

Though these explanations should not sufficiently account for some of the circumstances in our experiments, the following conclusions will not be the less correct.

1. The simultaneous division of both nerves of the eighth pair, is always mortal.

2. This division occasions an asphyxia, which lasts for a longer or shorter time according to the kind of animal submitted to this experiment.

3. This asphyxia is characterized by the same symptoms with the affection which occurs after the division of the nerves, and especially by the black coaly color of the blood both in the arteries and veins.

4. Whilst this asphyxia lasts, the air continues to enter the lungs, and the blood to circulate through them, which affords a convincing proof that this disorder does not arise from a suspension of the motions of the chest, or of the heart, but from the suspension of the action of the lungs.

5. It is possible by the mere compression of the eighth pair of nerves, regulated so as to be alternately continued or suspended for a short time, to produce or remove this asphyxia, and even to render it fatal. Hence when it follows the division or compression of the before mentioned nerves, it can be attributed to no other cause than the lesion of these nerves.

6. As the lesion of the eighth pair of nerves interrupts respiration, this function in the healthy state is under the influence of the nerves distributed to the lungs; under that of the brain whence the nerves are derived; and consequently of life, of which the action of the nerves and the brain, are only a condition.

There is then a primary cause of respiration, as well as of the other functions, dependent on life, which regulates its action, and determines the law and play of chemical affinities.

We must begin with this primary cause in our inquiries respecting respiration, and all the other functions, if we would judge correctly of the physical, mechanical, and chemical phenomena, and their various changes.

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*Case in which the operation of tying the Vena Saphena, for the cure of an old Ulcer in the Leg, terminated fatally.*

By HENRY OLDKNOW, Esq. Surgeon, Nottingham.

From the Edinburgh Medical and Surgical Journal, for 1809.

I BEG leave to offer, through the medium of your Journal, the following unfortunate result, from a case in which the saphena vein was tied, for a cure of a long standing ulcer of the lower extremity; and I make this communication to the public, because a correspondent in your last October number has spoken of the success of the operation, in a case which has lately occurred to him, and wherein he wishes to know the experience of other practitioners. At the same time, allow me to express a wish, that the report of unsuccessful cases were more frequently made, as no doubt they do sometimes occur; for I do think they would operate as much to the advantage of the healing art, and of our professional brethren, as the relation of such very extraordinary cases of successful practice, which we now and then meet with in some periodical publications. A young man (about twenty-three years of age) of plethoric habit, had been troubled, for the last two or three years, with painful ulcers about the inner ancle of the right leg, which repeatedly healed, and broke out again, so as to prevent, in a great measure, his attendance to business; he, therefore, became desirous, on my representation, of having the saphena vein tied, it being varicose to within a hand's breadth of the knee. The operation was performed in the following manner. An incision three quarters of an inch long, and about one inch and an half above the distention, was made through the skin immediately over the vein, and in the direction of its course. The cellular substance was then carefully divided, the vein lying deeper than usual, until its upper surface was completely exposed. Its accompanying nerve was cautiously avoided, be-



ing distinctly seen about four lines distant from it, and nearer to the spine of the tibia. A probe, armed with a double ligature, was passed under the vein, taking care to include nothing with it, it being cleanly dissected from the surrounding cellular membrane, upwards and downwards, as far as the extent of the first incision, and then tied. The portion of vein between the two ligatures being afterwards cut out, the lips of the wound were brought together by a suture and adhesive plaster, with a view of healing it by the first intention. My reason for using a suture, was to prevent the edges of the wound from curling too much inwards, (a circumstance I have seen often occur, when the sticking plaster alone is used,) thereby preventing its healing, or be the means of forming an irregular cicatrix. The patient was then put to bed, and directed to use a cold embrocation. On the second day he took some opening mixture. Third day, the wound was dressed, and appeared wholly united, except where the ligatures came out. The suture was cut out, and fresh adhesive plaster applied. On this day he complained of pain in the lower part of the limb, that is, from the lowermost ligature, along the vein, down the foot; to use his own expression, as if the blood in the vein was endeavouring to overcome the obstruction caused by the ligature. This sensation was, however, entirely removed, by applying a bandage moderately tight, from the toes, upwards. Fifth day, there appeared a little erythematous blush about the wound; he had a trifling epistaxis; and I thought proper to take from him sixteen ounces of blood, which was not inflamed; the purging mixture was repeated. Sixth day, complained of a little pain on the inner side of the knee, in the course of the vein; but there was no external inflammation at that part; the lips of the wound began to separate. In the evening, he was suddenly seized with a violent rigor, succeeded by a hot fit, and symptoms of great vascular action, and some tendency to delirium. Pulse 130, hard and full; therefore, sixteen ounces of blood were taken from the arm; it gave instantaneous relief, and was followed by the sweating stage, which continued several hours, and was succeeded by a state of quietude, soon, however, interrupted by a recurrence of the same train of violent symptoms, at first about once in twenty-four hours, but

gradually increasing in frequency, and diminishing in strength, until nature became exhausted, death closing the scene twenty-two days from the operation. For the first four or five days after the rigor, the wound discharged pretty freely, and by making strong pressure about the knee, a little matter was forced from it. The inflammation crept gradually up the vein, which was evident from its peculiar cordlike feel, and from giving pain on pressure, until it reached the groin, the inferior part getting well as the superior became bad, so that the wound was nearly healed before death, the ligatures having separated about the fourteenth day. There was no tumefaction of the cellular membrane, no enlargement of the glands in the groin, no superficial inflammation on the thigh. There was, it is true, a slight redness of the skin when the poultice was removed, (for the thigh, along the course of the vein was covered with cold poultice,) which entirely vanished on exposure to the air. The medical treatment was strictly antiphlogistic; the patient was repeatedly bled, and with apparent relief every time, the blood being extremely sizzly ever after the first rigor. Two days, however, previous to his death, the vital principle was so exhausted as to need the use of cordials.

This is the result of an operation, which, I believe, is generally considered as a very trifling one, and not endangering the life of the patient: judge, then, my mortification at this unexpected termination. On inquiry, however, I find another fatal case has occurred in this neighbourhood, differing in its symptoms from the one I have related. Large collections of matter formed in the cellular membrane, along the course of the vein, as far as the groin, and the patient died two months after the operation, the fever assuming the form of an intermittent. In this case, the incision was made transversely, and the vein was tied by a single ligature, as mentioned by your correspondent. Many are the cases, where much inflammation has followed the operation, and no doubt many persons undergo the operation without any such occurrences; but even then, it is a question, whether it be attended with that permanent advantage we are led to expect from it. Ought we, therefore, in any such cases, to run the risk of the danger incurred by the operation? Mr. Home, in his book on ulcers, mentions



symptoms of inflammation having taken place in a few instances; but he is more inclined to attribute them to the unhealthiness of large hospitals, than to any immediate effect of the operation; and in my opinion, his book is calculated to impress the minds of medical men, with too little importance in regard to the operation, and its consequences. In the above case, you will perceive the inflammation was confined to the vein itself. Are we, then, with Mr. Hunter, to attribute the death of the patient to the inflammation extending to the cavities of the heart, or to an inflammatory exhalation from the inner surface of the vein, which, being carried into the circulation, acts as a constant source of irritation to the heart and brain, and thus produces the disease? I think, at least, such an opinion is not irrational, knowing as we do, the deleterious effects which all the animal secretions, when injected into the veins, have upon the system.

Nottingham, February 11, 1809.

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*An account of a peculiar Disease of the Heart.*

BY DAVID DUNDAS, ESQ.

Sergeant surgeon to his majesty.

From the Medico-Chirurgical Transactions, Vol. I.

THERE is a disease of the heart, which I apprehend is not very uncommon, no less than nine cases of it having, in the course of thirty-six years, fallen under my care. I have also heard of several other cases, and yet I do not believe any account of it is to be found in any medical author.

The patient complains of great anxiety and oppression at the præcordia; has generally a short cough, and a difficulty of breathing, which is so much increased by motion or by any exertion, as to occasion an apprehension that a very little additional motion would extinguish life. There is also frequently an acute pain in the region of the heart, but not always.

The difficulty of breathing is also aggravated by taking even a small quantity of food.

He prefers lying on the back, complains of great palpitation of the heart, and violent pulsation of the carotid arteries, attended with noise in the ears and giddiness of the head.

In some cases I have found the action of the heart so very strong as to be distinctly heard, and to agitate the bed the patient is in so violently, that the pulse of the patient could be counted by looking at the motion of the curtains of the bed.

The pulse is always very quick, and is often irregular: in some cases it has been weak, but more commonly very hard.

Towards the conclusion of the disease symptoms of water in the chest take place, the legs become œdematous, and frequently a considerable collection of water is accumulated in the abdomen.

In all the cases which I have seen, this disease has succeeded one or more attacks of rheumatic fever. In one case the affection of the heart appeared at the commencement of the rheumatic fever, and its action was so rapid, that the pulse could not be counted for many days; much difficulty of breathing and oppression attended with a sense of great debility took place, and the inflammation, pain, and swelling of the extremities, after having shifted from one joint to another for many weeks, subsided; but the affection of the heart continued, generally attended with great pain, producing in the progress of the disease, and towards its close, a considerable disposition to dropsy, under which the patient lingered for ten months.

All those I have seen afflicted with this disease were young persons; only two were above twenty-two years of age. Six of them were males, and three females.

Most of them struggled with the disease for many months. Seven of the nine have died. One I am attending at present, and I think cannot recover; and one is apparently well, having survived the attack four years. He has had no rheumatic affection for two years and a half, but the action of the heart is still very violent and easily increased by exercise. His recovery is attributed to a very strict adherence for a long time to a vegetable and milk diet, and great attention to avoid any considerable exertion.

Of the seven cases which proved fatal, six have been opened, and all of them agree in the general appearance of the heart. In all the heart was uniformly found to be enlarged, in some, the enlargement was much more considerable than in others. In one case water was found in the pericardium, in



all the others the pericardium adhered to the heart. The left ventricle, in all the cases, was most enlarged in size, but not in thickness, and in most of them the heart was found of an unusually pale color, and very soft and tender in its texture.

In one case, the examination of which was made by Mr. Chilver, in the presence of Sir Walter Farquhar, Dr. Baillie, Dr. Saunders, Dr. Gillan, and myself, Dr. Baillie thus describes the appearance of the heart. "The pericardium was found closely adhering in every part to the surface of the heart. The heart itself had increased wonderfully in size; it was at least three times the size of a healthy heart. The muscular structure was, however, not increased in thickness beyond what it commonly is, so that its powers of action were not augmented proportionably to its bulk. As the quantity of blood in this heart was much larger than is natural, (for instead of a few ounces it contained almost a quart of blood) its powers of propelling this blood to the different parts of the body must have been much diminished below the common standard."

Mr. Thomas, of Leicester-square, gives the following account of the examination of the body of a young gentleman (who died of this disease,) at which I was prevented from being present. "The heart was somewhat larger than common, and the enlargement was confined principally to the left side. It was of a pale color, extremely soft and tender in its texture, and its cavities were filled with coagulated blood, having but a slight cohesion of its component parts. Nothing remarkable presented itself in the right auricle and ventricle; but upon opening the left ventricle was found an irregular excrescence of the nature of Polypus, attached to, and nearly occupying the whole of one of the *valvula mitrales*."

I have an account of a case so far back as the year 1770, in which all the appearances correspond almost exactly with those in the case described by Dr. Baillie. The heart is mentioned to have been three times its usual size; the pericardium adhered to the left ventricle, which was inflamed, and was thinner than the right. Where the pericardium did not adhere, a small quantity of water was found in it.

I examined lately, the body of a young lady who died of this disease, in presence of Mr. Nixon and Mr. Jones, sur-

geons, at Hampton, and found the heart of an unusual size, and the pericardium adhering closely in every part to it. The substance of the heart was of a pale color, and the texture of it was so tender, that the finger could with great ease be pushed through it. It was chiefly enlarged on the left side, but its muscular structure was not increased in thickness. The *valvulæ mitrales* were edged with a substance of a spongy appearance, perhaps coagulable lymph.

I have an account of an incipient attack of this disease, judiciously and successfully treated by Dr. Pemberton. His account of the case is so very distinct, and in many respects corresponds so exactly with the disease I have described, that I will take the liberty to give it in the doctor's own words.

"Mr. — had been in his youth, and indeed even to the time he was taken ill, (aged 36) subject to the acute rheumatism. He had been particularly troubled with this complaint during the whole of the winter immediately preceding the affection of the heart, which took place in March. He was seized with a considerable pain at the heart, and a difficulty of respiration, great palpitation and great anxiety. He conceived that the smallest motion of the body would have instantly destroyed him, and this dread seemed to have totally bereft him of the power of utterance. He sat for six or eight hours without being able to articulate a sound, though he was conscious of what was going on about him. He had frequent rigors, and almost constant profuse sweats. Cordials of various sorts were given him, till he appeared in a slight degree more composed, when a small quantity of blood was taken from the arm. This did not appear buffy. In about three days he ventured to move from the chair where he had remained from the first attack, but upon the slightest exertion the palpitation and anxiety were renewed. When he remained perfectly at rest the palpitation was hardly observable.

"In about a fortnight all his apprehensions of death seemed removed; but still on the most trifling exertion he was reminded of his former sufferings, so that for a month he scarcely moved at all.

"By placing the hand upon the heart, there appeared a very



great throbbing, which beat up the carotids so much as to occasion a most unpleasant noise in the head.

"On considering the circumstances of this case, I was inclined to suspect that a small portion of the surface of the heart had been affected with inflammation, and that in consequence, a partial adhesion had taken place between it and the pericardium. The plan recommended, was a seton in the region of the heart, and a pill composed of three grains of the *succus spissatus cicutæ*, and half a grain of the powder of *digitalis*, three times a day, abstinence from all fermented liquors, and a moderate quantity of animal food.

"The irregular action of the heart continued at intervals for eight months, when it gradually lessened, so that in about a year there remained no symptoms of the former complaint, and Mr. — is now (at the distance of four years) equal to undergo any exertion or fatigue without producing any irregularity of the heart's action."

The knowledge that this disease is always the consequence of, or is connected with, rheumatic affection, points out the necessity of attending to the translation of rheumatism to the chest; and shows the importance of employing very vigorous measures to remove it as soon as possible; but whenever it has made any considerable progress, I fear it will baffle every effort.

November 20th, 1806.

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Since writing the above I have been favoured by Dr. Marcet with an account of two cases of translation of rheumatism from the extremities to the chest, producing several of the symptoms I have noticed; but not in so marked a degree as in the cases I have seen. Both these patients died, and were opened, and in both of them the heart was found to be much enlarged. I also last year attended a person aged 29, who had twice had the acute rheumatism; but having been wet through last September, was seized with shiverings, succeeded by a pain across the chest, great difficulty of breathing, which was increased by the slightest motion, a very strong palpitation of the heart, and violent action of the carotid arteries, accompanied with a sense

of great debility, and an apprehension she was just going to expire.

She had been in this state for a fortnight, before I saw her. She had not been able to go to bed for many nights; her legs and thighs were much swollen, and her pulse, which was very weak, was so quick, that it could not be counted. She had no cough. She submitted to have a blister applied to the region of the heart, but would not allow an issue to be made. By the use of the digitalis her pulse became less frequent; but as it disordered her head and stomach she was obliged to relinquish it. The action of the heart now was tremendous; she daily became weaker, although her appetite continued good to the last, and she died at the end of two months.

She was opened in the presence of Mr. Taylor, of Kingston. The lungs on the left side had very little space to act in, the heart, which was enlarged to a most extraordinary size, occupying the greater part of the left side of the thorax. The lungs were found on both sides, to have strong adhesions to the pleura; and above a pint of water was found in the cavity of the thorax. A considerable quantity of water was contained in the abdomen. All the abdominal viscera were sound, except the spleen, which was of a much paler color and softer texture than usual.

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*The Case of a Man who died in consequence of the Bite of a Rattle Snake, with an Account of the Effects produced by the Poison.*

By EVERHARD HOME, Esq. F. R. S.

WITH PRELIMINARY OBSERVATIONS.

From the London Medical and Physical Journal, for 1810.

THE public sensation which was occasioned by a man having been bitten by a *rattle snake*, kept at a menagerie in Piccadilly, was extended as far as a knowledge of the fact had spread. Curiosity, with respect to the symptoms and fate of the wounded man, was highly excited; but many months elapsed before this curiosity was satisfied. A minute detail of the case has now been published, and we embrace this opportunity of laying it before our readers.



The genus to which the serpent that wounded the subject of the following case, belongs, is peculiar to America, and has the expressive appellation, CROTALUS, given to it by Linnæus. It contains five species, according to the *Systema Naturæ* Linn.

- 1 CROTALUS *miliarus*.
- 2 ——— *horridus*.
- 3 ——— *dryinas*.
- 4 ——— *durissus*.
- 5 ——— *mutus*.

To which the Count La Capede has added a sixth, the Fisher, (*le Piscivoire*) described by Catesby, (*Natural History of Carolina*), and by him only, and two varieties, Gronian *durissus*. (*Crotalophorus tertius*. Gron. Mus. 11. 70. n. 45,) and the fasciated *durissus*. (*Crotalus fasciatus*. Vosmaer. monagr. anno 1767.) The whole of the species of this genus are furnished with poisonous fangs, but have the character of seldom biting,\* unless irritated, or for the purpose of securing their prey.

The species by which this person was bitten, is the *crotalus horridus*, the most terrific and formidable of the genus. Many names have been given to it, either expressive of its mechanism or its qualities; it is the *caudisona terrifica* of Laurentius; the *vipera caudisona*, and *anguis crotalophorus* of our countryman, Ray; the *regina serpentum* of Hernandez; the *boiquira* of Piso, and the *Encyclop. Method.* From the quickness of its motions, the Mexicans call it *ecacoatl*,† for it darts on its prey, wounds, and retires with the swiftness of the wind.

The *crotalus horridus* is found along the American continent, from the Straights of Magellan, to Lake Champlain, in Canada. For a long time after the discovery of the new world, this *regina serpentum*, reigned without control over its vast wilds and impassable forests. Few, possibly none, of the native animals preyed upon it; and it was secured from the destroying hand of man, by superstitious terrors that influenced, un-

\* The pacific disposition of the rattle snake is asserted by Batram.

† This word, expressive of velocity, is sometimes written *Hoatcoatl*. Savage nations often employ a highly figurative language. The Mexicans denominate the rattle snake *Tuetlatcauqui*, the king of snakes.

accountably indeed, the ancient Americans. As human dominion extends, the lower orders of nature submit, or are exterminated. Thus, the arts and labour of civilized life, as they clear and cultivate the soil of America, daily diminish the numbers of the *crotalus*; and contract the space in which its ravages are exercised. It is probable the time is not far distant, when it may become a rare animal even in America.\*

The habits and powers of the rattle snake, (*crotalus horridus*, for it is this species of the genus, upon which observation has been most exercised,) have been examined with anxious precision; and much has been written to elucidate them; but on some of its striking properties, doubt still remains. There are four particulars, which have most employed attention: its poison; its power of fascination; its rattle; and its abdominal fœtor.

The general terror excited by the rattle snake, is described with wonderful force and picturesque effect, by the elegant Buffon. "A traveller," says he, "wandering in the burning deserts of Africa, who hears the roaring of a famished tiger, furious for prey, and who has no means either of escape or defence, feels not greater dread than when passing through the vast forests of the warm and humid regions of the new world, he scents the abdominal fœtor of the *boiquira*, hears the terrifying noise of its portentous rattle, and observes it ready to spring upon him from amid the verdant foliage and brilliant flowers of those delightful solitudes." Again, he says, "During storms of thunder it is most to be dreaded. One even shudders in idea, at the terrible situation and mortal agony of a person, who, escaping in the midst of almost total darkness from the terror of a tornado, seeks shelter under the edge of a rock, and perceives the glaring eyes of a *boiquira*, through the gloom, sometimes illuminated by the lightning, while he hears the rattle of its tail, and the horrid hissings of its venomous mouth. Language has not power to express the deadly swiftness with which the poison of the *boiquira* sometimes acts. In

\* It is a remarkable circumstance, that the European hog should be the most formidable instrument for the destruction of this serpent. The moment a hog scents a rattle snake, he erects his bristles, rushes upon it with avidity, and seizes it with extraordinary address.



a minute, half a minute, in an instant, the bitten animal has expired."\* In this country, a rattle snake bit dogs that died in a few minutes. These experiments were made at the college of physicians, about 1728. Mr. Ranby, the surgeon, examined the poisonous apparatus, and gave a description of it in the Philosophical Transactions, vol. xxxv. p. 377. Dr. Edward Tyson had, some years before this, made a minute dissection of another that had been sent to this country, about 1690; and which probably was the first brought hither alive. (Philosophical Transactions, vol. xiii. p. 25.) Of the chemical qualities of the venom not much is known. It is described, however, as a greenish fluid, and that it tinges linen of a green color, which becomes brighter by washing. It is believed, that the poison sometimes accumulates in its receptacle, to a degree that becomes painful to the animal, and that then it attacks every thing that comes in its way. (Hist. Gen. de Voyage.)

Of the fascinating power of the *crotalus horridus* much has been related, but accompanied with circumstances so extravagant, as to gain but little credence with the reflecting part of mankind. An ingenious and learned naturalist of America, (Dr. Barton) has written a treatise expressly on the subject, in which he adduces many observations to prove the nonexistence of this power. The fascinating property has been supposed by some to be connected with the stupifying quality in the breath of the *boiquira*; by others, to be effected by the dazzling splendor of its eye. *Kalm* offers a solution of some of the instances. He conjectures, that "in most of those instances where a squirrel or a bird has been seen to come, apparently of its own accord, from the top of a tree into the mouth of the rattle snake, that it had been previously bitten by the serpent; and having escaped into the tree, where it expressed, by its cries and agitation, the violent action of the venom; that as its strength diminished, it gradually descended from branch to branch, till at length it became exhausted, and fell to the ground, near the serpent, which remained steadfastly following all its motions with its eyes, and ready to seize it whenever it came within reach."

\* In the case related by Mr. Home, it may be observed, though the patient lived some weeks, the sensorium was immediately affected.

The mechanism of the rattle has been minutely described; and its structure is doubtless a subject worthy of investigation; but why the animal should have an appendage that must necessarily give notice of its hostile intention, is a question not easily solved. It has been understood, that this serpent could not move without its rattle giving notice of its being in action. This did not, however, appear to be precisely the fact, in the two rattle snakes in Frazer's nursery, Chelsea, in the summer of 1809, and by one of which, the man, whose case is here related, was bitten. From observing the habits of those, we can say that when irritated, they formed themselves into several coils, the head erect in the centre, and the tail, with the rattle at the circumference. In this position, the rattle was moved with a quick and tremulous motion, to be heard at thirty yards distance. This motion of the rattle, which is effected with extreme velocity, appears to mark the design to strike when the object comes within the proper distance; and certainly manifests the snake then to be in a hostile position. If in the common movements this apparatus gives any audible sound, it will have a different character from that which indicates anger. The stroke of the snake, and the cessation of the action of the rattle, occurred at the same instant.

The fetid odor that is emitted from this serpent, if it sometimes deprives small animals of the power to escape; at others, serves as a warning of its approach; and horses, cattle, and other animals thus escape from its bite. This disgusting scent has been described as a property in the breath, and exhaled from the lungs; it is probable, however, that it is afforded by some glands described by Tyson, and placed near the anus: or is compounded of the fœtor arising from the fluid secreted by these glands, and the halitus thrown out by the lungs and general surface.

Many remedies for the bite of the rattle snake have been recommended, particularly the *polygala senega*; and several instances are recorded of their success. In the case before us, those employed were given on general indications, rather than with a view to specific properties.

When we request the attention of our readers and correspondents to a subject of such interest and importance as the



*animal poisons*, and especially those of the order of *serpentes*, we expect that the natural history and deleterious effects of those poisons will be elucidated in a proportion agreeing with the investigation we hope to elicit.

*The Case of a Man who was bitten by a Rattle Snake, &c. &c.*  
(Philosophical Transactions, for 1810. Part I.)

THOMAS SOPER, twenty-six years of age, of a spare habit, on the 17th of October, 1809, went into the room in which two healthy rattle snakes, brought from America, in the preceding summer, were exhibited. He teased one of them with the end of a foot rule, but could not induce the snake to bite it, and, on the rule dropping out of his hand, he opened the door of the cage to take it out; the snake immediately darted at the hand, and bit it, twice in succession, making two wounds on the back part of the first phalanx of the thumb; and two on the side of the second joint of the fore finger. The snake is between four and five feet long, and when much irritated, bites the object twice, which I believe snakes do not usually do.

The bite took place at half past two o'clock. He went immediately to Mr. Hanbury, a chemist in the neighbourhood. There was at that time no swelling on the hand, and the man was so incoherent in his language and behaviour, that Mr. Hanbury considered him in a state of intoxication, and gave him a dose of jalap, to take off the effects of the liquor, and made some slight application to the bites. It appeared, on inquiry, that the man had been drinking, but that before he was bitten, there was nothing unusual in his behaviour. After leaving Mr. Hanbury's, the hand began to swell, which alarmed him, and he went to St. George's hospital. He arrived there at three o'clock. The wristband of his shirt had been unloosed, and the swelling had extended half way up the fore-arm, before his admission. The skin on the back of his hand was very tense, and the part very painful. At four o'clock, the swelling extended to the elbow, and at half past four, it had reached half way up the arm, and the pain had extended to the axilla. At this time, Mr. Brodie, who visited him in my absence, first saw him; he found the skin cold; the man's answers were incoherent: his pulse beat one hundred strokes in a minute,

and he complained of sickness. Forty drops of aqua ammoniæ puræ, and thirty drops of spiritus ætheris vitriolici, in an ounce of mistura camphorata, were given to him, but did not remain on his stomach. The wounds were bathed with the aqua ammoniæ puræ, and the arm and forearm had compresses wetted with camphorated spirits applied to them. At five o'clock, he took two drachms of spiritus ammoniæ compositus, and thirty drops of ether, in an ounce and an half of mistura camphorata, which remained on his stomach. At six o'clock, his pulse was stronger; at half past seven, his pulse was very feeble, and thirty drops of ether, and the same quantity of aqua ammoniæ puræ were given in water. At half past eight it was repeated. At nine o'clock he had the feeling of great depression; his skin was cold, his pulse weak, beating eighty strokes in a minute. The dose was increased to fifty drops of both medicines, and repeated. At a quarter past ten o'clock, the pain had become very violent in the arm; his pulse was stronger, but fits of faintness attacked him every fifteen minutes, in which the pulse was not perceptible, but in the interval his spirits were less depressed. In the course of the evening he had two stools. At half past eleven o'clock I first saw him. The hand, wrist, forearm, and arm were much swelled up to the top of the shoulder, and into the axilla. The arm was quite cold and no pulse could be felt in any part, not even in the axilla, the swelling preventing me from feeling the axillary artery with any degree of accuracy. The wounds made on the thumb were just perceptible; those on the finger were very distinct. His skin generally was unusually cold. I took some pains to diminish his alarm of danger, and found his mind perfectly collected: he said, he hoped he should recover. At one o'clock in the morning of the 18th, he talked indistinctly: his pulse beat one hundred in a minute; the attacks of faintness came on occasionally. The medicine was repeated every hour. At eight o'clock, on the morning of the 18th, his pulse beat one hundred and thirty-two strokes in a minute, and was very feeble. The swelling had not extended beyond the shoulder to the neck, but there was fulness down the side, and blood was extravasated under the skin as low as the loins, giving the back, on the right side, a mottled appearance. The whole arm and hand were cold, but painful when pressed; the



skin was very tense; on the inside of the arm below the axilla, and near the elbow, vesications had formed, and under each of the vesications there was a red spot in the cutis, of the size of a crown piece. The skin generally, over the body, had become warm. He was low and depressed; there was a tremulous motion of his lips, and the faintings recurred at nearly the same intervals as in the preceding evening. The last dose of medicine was rejected by vomiting, but some warm wine remained on his stomach. The arm was fomented. At twelve o'clock, in addition to the above symptoms, there was a starting of his limbs. He had attempted to take some broth, but his stomach did not retain it. The skin of the whole arm had a livid appearance, similar to what is met with in a dead body, when putrefaction has begun to take place, unlike any thing which I had ever seen in so large a portion of the living body. An obscure fluctuation was felt under the skin of the outside of the wrist, and forearm, which induced me to make a puncture with a lancet, but only a small portion of a serous fluid was discharged. My colleague, Dr. Nevinson, was present at this visit, and we agreed to continue the internal use of the volatile alkali, with the view of rousing the stomach to action, not considering it as having any specific power over the poison. At eleven o'clock in the evening, finding that his stomach did not always retain the medicines, nor even small quantities of brandy, which were given him, I directed the volatile alkali to be left off, and two grains of opium to be given, and repeated every four hours. At this time his pulse was scarcely perceptible at the wrist; the fainting fits were not less frequent. The vesications and red spots were increased in size.

October 19. At nine o'clock in the morning his pulse was scarcely perceptible: his extremities were cold; the vesications were larger, and the size of the arm was diminished. He was drowsy, probably from the effect of the opium. He had taken nothing but brandy during the night. At three o'clock in the afternoon he was more depressed: spoke only in whispers: the vesications were increased: the fainting fits less frequent. The arm was diminished in size; and he had sensation in it down to the fingers. At eleven o'clock at night, his pulse beat one hundred and thirty in a minute, and was low. The opium was

left off. A stool was procured by clyster. He was ordered to have a glass of camphorated mixture occasionally, and wine and brandy, as often as he could be induced to take them.

October 20. He had dozed at intervals during the night; his spirits were better, and his extremities warmer. At nine o'clock he took coffee for breakfast. He afterwards took some fish for dinner, but it did not remain on his stomach; he therefore took brandy and coffee at intervals, half an ounce at a time, as larger quantities did not remain on his stomach.

October 21. He had slept at intervals during the night, but was occasionally delirious: his pulse one hundred and twenty in a minute. Brandy and jelly were the only things that stayed on his stomach. The size of the arm was reduced; but the skin was extremely tender.

October 22. He had slept during the greater part of the night; his pulse beat ninety-eight in a minute; he took some veal for dinner, and brandy at intervals. In the evening, his pulse became full and strong; he was ordered wine instead of brandy. The right side of the back, down to the loins, was inflamed and painful, and had a very mottled appearance, from the extravasated blood under the skin.

October 23. His pulse continued full; and the arm was very painful, though reduced in size. The vesications had burst, and the exposed cutis was dressed with white ointment. Stools were procured by an opening medicine. He took some veal and porter for dinner; the wine was left off. In the evening, he had a saline draught, with antimonial wine.

October 24. There was no material change.

October 25. His pulse had increased in frequency; but in other respects he was nearly the same. His bowels were opened by medicine.

October 26. The arm was more swelled and inflamed.

October 27. The inflammation of the arm had increased: his tongue was furred, and his pulse was very frequent. He attempted to sit up; but the weight of the arm and the pain prevented him. The arm was bathed with spirits of wine and aqua ammoniæ acetatæ in equal quantities.

October 28. A slough had begun to separate from the inside of the arm below the axilla, and a purging had come on, for



which he was ordered chalk mixture and laudanum. In the night he had a rigor.

October 29. The purging had abated: his pulse beat one hundred in a minute, and was feeble. A large abscess had formed on the outside of the elbow, which was opened, and half a pint of redish brown matter was discharged, with sloughs of cellular membrane floating in it. The lower part of the arm became much smaller, but the upper part continued tense. A poultice was applied to the wound. The lower portion of the arm and the forearm were covered with circular strips of soap cerate. He was ordered to take the bark, and allowed wine and porter.

October 30. The redness and swelling of the upper part of the arm had subsided: the pulse was one hundred in a minute. The purging had returned. The bark was left off: the chalk mixture and laudanum were given, and an opiate clyster administered.

October 31. The pulse beat one hundred in a minute; the discharge from the abscess had diminished; the purging continued, and at night he had a rigor.

November 1. The pulse was one hundred and twenty. His voice was feeble; he had no appetite, was delirious at intervals. Ulceration had taken place on the opening of the abscess, so that it was much increased in size. He drank two pints of porter in the course of the day.

November 2. His pulse was very weak; his countenance was depressed; his tongue brown; the ulceration had spread to the extent of two or three inches. Mortification had taken place in the skin nearer the axilla. His stomach rejected every thing but porter; in the night he was delirious.

November 3. The mortification had spread considerably: the purging continued: the forefinger, which had mortified, was removed at the second joint.

November 4. He died at half past four o'clock in the afternoon.

Sixteen hours after death, the body was examined by Mr. Brodie and myself, in the presence of Mr. Maynard, the house surgeon, and several of the pupils of the hospital.

With the exception of the right arm, which had been bitten,

the body had the natural appearance. The skin was clear and white, and the muscles contracted.

The wounds made by the fangs at the base of the thumb were healed, but the puncture made by the lancet at the back of the wrist was still open. That part of the back of the hand, which immediately surrounded the wounds made by the fangs, for the extent of one inch and an half in every direction, as also the whole of the palm, was in a natural state, except that there was a small quantity of extravasated blood in the cellular membrane. The orifice of the abscess was enlarged, so as to form a sore on the outside of the arm, elbow, and forearm, near six inches in length. Around this, the skin was in a state of mortification, more than half way up the outside of the arm, and as far downwards, on the outside of the forearm. The skin still adhered to the biceps flexor muscle in the arm, and flexor muscles in the forearm, by a dark-colored cellular membrane. Every where else in the arm and forearm, from the axilla downwards, the skin was separated from the muscles; and between these parts there was a dark-colored fluid, with an offensive smell, and sloughs of cellular membrane, resembling wet tow floating in it. The muscles had their natural appearance every where, except on the surface, which was next the abscess. Beyond the limits of the abscess, blood was extravasated in the cellular membrane; and this appearance was observable on the right side of the back as far as the loins, and on the right side of the chest over the serratus major anticus muscle.

In the thorax the lungs had their natural appearance. The exterior part of the loose fold of the pericardium, where it is exposed, on elevating the sternum, was dry, resembling a dried bladder. The cavity of the pericardium contained half an ounce of serous fluid, which had a frothy appearance from an admixture of bubbles of air. On cutting into the aorta, a small quantity of blood escaped, which had a similar appearance. The cavities of the heart contained coagulated blood.

In the abdomen, the cardiac portion of the stomach was moderately distended with fluid: the pyloric portion was much contracted; the internal membrane had its vessels very turgid with blood. The intestines and liver had a healthy appearance.



The gall bladder was moderately full of healthy bile. The lacteals and thoracic duct were empty; they had a natural appearance. In the cranium, the vessels of the pia mater and brain were turgid with blood; the ventricles contained rather more water than is usual; and water was effused into the cells, connecting the pia mater and tunica arachnoides. It is to be observed, that these appearances in the brain and its membrane are very frequently found in cases of acute diseases, which terminate fatally.

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The following cases were sent from India, to my late friend, Dr. Patrick Russel; they arrived after his death, and Mr. Claude Russel very kindly gave them to me, knowing the subject of them to be one in which I had taken an interest. As they correspond in many of the circumstances, with that which has been detailed, I have inserted them in this place, as well as an experiment, which I had an opportunity of making in the West Indies, on the effects of the snake's poison on animals.

A boy, a slave of a gentleman in India, was bitten by a snake, called Kamnlee, by the natives, in the lower part of the arm, at eight o'clock in the evening. The blood flowed very freely for some time. He died next day at noon, in great pain.

A seapoy, sixty years of age, was admitted into the hospital of his regiment, under the care of Mr. Perrin, assistant surgeon, at four o'clock in the afternoon, of the 15th of October, 1802, in consequence of his being bitten by a cobra di capello, on the back part of the hand. At the time of his admission, he complained of pain running up the arm. He immediately took a drachm of eau de luce; and this dose was repeated every half hour; and the same remedy was applied externally as a lotion to the arm and forearm. At four o'clock in the morning of the 16th of October, the pain began to increase, and the arm to swell with great hardness and stiffness, and tumor in the axilla, with much inclination to vomit. He took twelve grains of Dr. James's powder, which brought up a great quantity of bilious matter. He drank copiously of warm water, but no perspiration was induced. He appeared relieved for a short time. At eight o'clock in the morning, the arm was distended, painful and discolored. He took four ounces of brandy, and repeated it every hour, until twelve o'clock, with a drachm of

eau de luce occasionally. At this time he was a little revived. The brandy was reduced to two ounces, which were carefully and regularly given every hour, until twelve at noon, on the 17th of October, when the arm was more free from pain, but much swelled, hard, and black: his spirits and pulse also were considerably relieved. The eau de luce was now omitted, but the brandy was continued every hour, until twelve o'clock at noon on the 18th of October, when the stiffness and tumor in the axilla had disappeared; the arm was still swelled, but was softer, and less painful. The brandy was omitted; at night he took six grains of Dr. James's powder. On the 19th of October, the arm was less, softer, with little or no pain; a blister was formed and burst on the back of the hand, which discharged three ounces of black fetid pus. On the 20th, an abscess burst on the hand, in the same situation as the blister, which discharged a large quantity of fluid, having an offensive smell. He was directed to take a drachm of Peruvian bark, in port wine, every two hours. On the 22d, the swelling was gone, but the discharge was considerable. From this time, the man gradually, but slowly recovered, with the loss of the use of his forefinger, which remained permanently extended, and some of the other fingers were affected in a less degree.

In this case, the swelling of the arm was slower in coming on, and less extensive; the pain running up to the axilla, which preceded it, was mistaken for the effect of absorption.

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In the year 1782, while in the island of St. Lucia, I made the following experiment.

A spotted dark-colored snake, about two feet in length, having the poison fangs on each side double, with the corresponding surfaces grooved, so as to form a canal for the poison, was put into a square tin box, open at the top, in which a half grown rat was confined. The rat expressed great terror, and remained crouching in one corner of the box, with its eyes fixed on the snake, which lay coiled at some distance; they were allowed to remain a few minutes in this situation. I then raised one end of the box, which caused the snake to slide along the smooth surface, till it came in contact with the rat, which it immediately bit. The rat died in a minute after the bite. I re-



moved it immediately from the box, by means of a long pair of forceps. The wounds made by the fangs were marked by two specks of blood immediately below the shoulder-blade. On dividing the skin with a scalpel, the cellular membrane under it was found entirely destroyed: the muscles were detached from the ribs, and from a small portion of the scapula. The parts immediately surrounding the bite were exceedingly inflamed; as far as I could trust to memory, the appearances very much resembled those produced on the muscles of a dog's thigh, by the application of white arsenic, in consequence of which death ensued in about sixteen hours.

Fifteen hours after the death of the first, a second rat was bitten by the same snake. The rat was much irritated, and bit the snake in the neck, so violently, that the latter died in about ten minutes. The rat continued very lively for about six hours, and then died. On examination after death, the bite was found to have been inflicted on the left side of the navel; and the abdominal muscles at that part were in the same state as in the other rat, but in a less degree.

It appears from the facts which have been stated, that the effects of the bite of a snake vary, according to the intensity of the poison.

When the poison is very active, the local irritation is so sudden and so violent, and its effects on the general system are so great, that death soon takes place. When the body is afterwards inspected, the only alteration of structure met with is in the parts close to the bite, where the cellular membrane is completely destroyed, and the neighbouring muscles very considerably inflamed.

When the poison is less intense, the shock to the general system does not prove fatal. It brings on a slight degree of delirium, and the pain in the part bitten is very severe; in about half an hour, swelling takes place from the effusion of serum in the cellular membrane, which continues to increase with greater or less rapidity for about twelve hours, extending during that period into the neighbourhood of the bite; the blood ceases to flow in the smaller vessels of the swollen parts; the skin over them becomes quite cold; the action of the heart is so weak, that the pulse is scarcely perceptible; and the stomach is so irritable that nothing is retained on it. In about sixty hours these symp-

toms go off; inflammation and suppuration take place in the injured parts; and when the abscess formed is very great, it proves fatal. When the bite has been in the finger, that part has immediately mortified. When death has taken place under such circumstances, the absorbent vessels and their glands have undergone no change similar to the effect of morbid poisons, nor has any part lost its natural appearance, except those immediately connected with the abscess.

In those patients who recover with difficulty from the bite, the symptoms produced by it go off more readily, and more completely than those produced by a morbid poison, which has been received into the system.

The violent effects which the poison produces on the part bitten, and on the general system, and the shortness of their duration, where they do not terminate fatally, has frequently induced the belief, that the recovery depended on the medicines employed: and in the East Indies, eau de luce is considered as a specific for the cure of the bite of the cobra di capello.

There does not appear to be any foundation for such an opinion; for, when the poison is so intense, as to give a sufficient shock to the constitution, death immediately takes place; and where the poison produces a local injury of sufficient extent, the patient also dies; while all slighter cases recover.

The effect of the poison on the constitution is so immediate, and the irritability of the stomach is so great, that there is no opportunity of exhibiting medicines till it has fairly taken place, and then there is little chance of beneficial effects being produced.

The only rational local treatment, to prevent the secondary mischief, is making ligatures above the tumefied part, to compress the cellular membrane, and set bounds to the swelling, which only spreads in the loose parts under the skin; and scarify freely, the parts already swoln, that the effused serum may escape, and the matter be discharged, as soon as it is formed. Ligatures are employed in America, but with a different view, namely, to prevent the poison being absorbed into the system.



## SELECTED REVIEWS.

*Observations on Fungus Hæmatodes, or Soft Cancer, in several of the most important organs of the Human Body; containing also a comparative view of the Structure of Fungus Hæmatodes, and Cancer, with Cases and Dissections. By J. Wardrop, F. R. S. E. Fellow of the Royal College of Surgeons, and one of the Surgeons to the Public Dispensary Edinburgh. Illustrated with Plates. Edinburgh, 1809. pp. 205.*

From the Edinburgh Medical and Surgical Journal, for April 1810

THE morbid structure, which forms the subject of these observations, has hitherto been very generally confounded with cancer. By some surgeons it has, indeed, been sometimes distinguished by the epithet of soft. Cases of this tumor occurring in the inferior extremities and shoulder were, however, first particularly noticed by Mr. John Burns, under the name of spongoid inflammation; and others have since been described in the extremities, and in the female breast, by the title of fungus hæmatodes, by Mr. Hay of Leeds. In the classification of Mr. Abernethy, it is distinguished by the name of medullary sarcoma, and is described to have been found not only in the extremities, but also in the testicle, and in various parts.

Much, however, was still wanting to complete the history of this tumor; and Mr. Wardrop, whose attention was particularly called to the subject, from having had an opportunity of observing an example of it, in its early stage, affecting the ball, and of tracing its whole progress to its fatal termination, has, in these pages, brought together a number of interesting facts and observations, which, if they do not complete the history, have enabled him to extend considerably our knowledge of the disease. The plan of his inquiry is stated to be,

“ To describe the appearances, and point out the character of fungus hæmatodes in some of the most important organs of the human body. As these may be more easily traced, more distinctly and satisfactorily seen during the whole

gress in the eye than in any other part, I shall first give a description of the disease in that organ; it will be next examined in the superior and inferior extremities, in the testicle, mamma, uterus, and ovarium, liver, spleen, lungs, and thyroid gland; in all of which its existence has been ascertained. Having done this, some general views of the change of structure in fungus hæmatodes will be pointed out, and a comparison made between it and the cancerous structure; the texture or textures in which it originates will also become an object of investigation; and, afterwards, such practical conclusions will be drawn from a review of all the facts in the history of the disease, as our data appear to warrant."

We shall now endeavour to lay before our readers a very abridged account of some of the most important of these observations. The numerous and ably selected cases which illustrate the text, and make the disease familiar to the intelligent reader, we must, for the present, pass over in silence.

The progress of fungus hæmatodes of the eyeball appears to be marked by the following appearances. Dilatation and immobility of the pupil, an amber or greenish color in the bottom of the eye, arising from a solid substance, which grows so as to occupy the whole posterior chamber of the eyeball, where it has sometimes been mistaken for cataract, and advances at last towards, and occupies, the anterior chamber; the cornea ulcerates; the eyeball becomes unequal and rugged; a fungous tumor shoots out, which rapidly acquires a great bulk; is generally of a dark-red or purple color, coated with coagulated lymph, readily torn, bleeding profusely, sloughing, and throwing out successive growths, and accompanied with tumor and inflammation of the parotid, of the maxillary, and other neighbouring glands.

On dissection, a tumor is found in the posterior chamber, extending from the entrance of the optic nerve forwards, and displacing and destroying the humors of the eye. This tumor has the appearance of medullary matter, a white opaque homogeneous substance, of the pulpy tenacity of brain, and sometimes as soft as custard, with filamentous cellular matter interposed. Though generally whitish, it has been found of a



reddish, and of a dark-brown color, and almost black. Bony or gristly particles have sometimes been observed in it. The sclerotic coat is little changed, but all the other parts of the eyeball are more or less disorganized. The retina has always been found completely changed; and the optic nerve, and sometimes the brain itself, are involved in the disease.

“In some cases the nerve retains its natural form, becoming thicker, much firmer and harder than natural, of a brownish ash color, and losing that tubular appearance which is observed in the healthy nerve with the naked eye. In other examples of the disease, the optic nerve, besides being altered in structure, is split into one or more pieces, the morbid growth filling up the intervening spaces surrounding the different portions of the nerve, and forming one connected mass with the contents of the eyeball. The divided portions of the nerve lose entirely their natural structure and color, becoming soft and pulpy, and sometimes of a deep yellow hue. In some cases the nerve has a flesh color, and more of the texture of sound liver. In many no distinction can be made between the neurilema and medullary portions, whilst, in others, changes take place in the one which are not observed in the other. In the case formerly mentioned, where the contents of the eyeball and external tumor had a very remarkable dark appearance, the neurilema remained unchanged, but the medullary portion of the optic nerve had the same dark-brown color, as far up as the nerve had been divided in the extirpation of the eyeball. After the patient's death, which was nearly five months after the operation had been performed, it was found, on dissection, that the same singular black appearance extended along the medullary portion of the optic nerve to beyond the union of the two nerves; and, what is an important fact to the physiologist, this case proved, in a very striking manner, that the optic nerves do not decussate each other. The nerve of the right eye arose from the right side of the brain, and was healthy all the way to where it formed the retina; whereas the left optic nerve was black, from the place where it had been divided during the operation of extirpating the eyeball, to beyond the union of the two nerves.”

One case only, that related by Mr. Hayes in the 3d vol. of the London Medical Observations and Inquiries, has come to the knowledge of our author, in which the retina or optic nerve appeared to be unchanged in structure. He is nevertheless inclined to think that the disease begins in the retina.

When the absorbent glands, in the progress of the disease, become enlarged, they are found to be converted into the same medullary looking matter; and when they ulcerate, a very unhealthy sloughy sore is produced.

Out of twenty-four cases of this disease in the eye, twenty of them were patients under twelve years of age, a proof of its dissimilarity to cancer, which is for the most part confined to those advanced in years. Vision is commonly lost even before any disease is remarked to exist by the parents of such children. In a great proportion of cases, the patient appeared to have received a blow, which brought on inflammation, and, a few days after, the colored substance became perceptible at the bottom of the eye. In others, a slight redness of the white of the eye may be perceived, with indistinct vision, slowly increasing, with uneasy aching pain, till the eyeball burst, and the fungus appear.

The conclusion from the whole is very melancholy. The disease has resisted the power of every mode of treatment; and the last resource, extirpation, has been equally unsuccessful. All, with the exception of one doubtful case, have terminated fatally. Probably the operation has been too long delayed, and performed not until the parts within the cranium were irrevocably diseased. Though the retina and optic nerve seem in every case very early affected, the earlier the eye is extirpated, the greater hope may be entertained of success. "It is an experiment, at all events, which well merits trial; and were I in any case to be assured of the existence of the disease in the early stage, I would have no hesitation in urging the performance of the operation."

In the extremities, fungus hæmatodes has been met with contiguous to the ankle and knee-joints, on the shin, calf of the leg, on the thigh, in the groin, and on the hip-joint, in the wrist, arm, forearm, and over the elbow and shoulder-joints. In these parts it has occurred at all the different periods



of life. The first appearance is that of a small movable tumor, smooth, equal, and firm, though not so hard as scirrhus. If deep seated, its commencement is less distinct; it advances slowly. When the swelling becomes large, it has a soft elastic feel; and a sense of fluctuation is sometimes so striking, that the swelling has been punctured to discharge the supposed contained fluid. But no fluid follows the operation. If the tumor be thus opened, or when the covering skin naturally ulcerates, a fungus begins to rise, which rapidly increases in bulk. The fungus is round, ragged, and unequal in its surface, of a dark-red color, easily torn, and bleeding on the smallest friction. It has generally a very narrow neck, and, in some cases, spreads out so much as completely to cover the edge of the ulcerated integuments. When the fungus acquires a very large size it sloughs, and renders the discharge extremely fetid, and hemorrhage often takes place. The degree of pain accompanying the disease is various. When the tumor is small the pain is trifling, but as it enlarges it becomes more tender; in some cases it has a sharp stinging pain darting through it; and, when the disease is far advanced, it is generally attended with acute pain.

In the progress of the disease, the absorbent glands, in the course of circulation, are contaminated and become enlarged; and, as the fatal termination approaches, the swelling of the glands, and the number affected are often prodigious, particularly in the inferior extremities; for here the glands, along the course of the iliac vessels and aorta, all contribute to form one formidable disordered mass within the abdomen.

On dissection of fungus hæmatodes in the extremities, the primary tumor exhibits the true medullary structure, and is commonly found to consist of distinct portions or lobes, separated by thin membranous intersections; color pale gray, or brownish red. Some portions, in color and texture, resemble brain; some are of a deep yellow, like the yolk of egg; and others resemble coagulated blood or liver. The affected absorbent glands acquire a structure similar to that of the original tumor.

All remedies are useless against this disease. The removal of the tumor by the knife has been in many cases attempted;

but in all the practice was unsuccessful, the disease afterwards returning at the place from which the tumor was removed. The only chance, therefore, of saving the patient's life, is by early amputation of the limb.

In the testicle the progress of the tumor is at first slow, and attended with little pain. When it has acquired a considerable bulk, it is soft and elastic, and communicates to the touch a sense of fluctuation so imposing, that it has been mistaken for hydrocele; from which it may be distinguished by the history of its rise and progress, by want of transparency, and by its greater comparative weight. When very large, the softness becomes more unequal, and, in some places, the tumor feels hard; the scrotal veins become varicose, and the inguinal glands and spermatic cord become diseased. In no case, within our author's knowledge, have the integuments of the scrotum given way; the irritation and fever, it is observed, created by the extent of disease, seem to cut off the patient before ulceration could take place. *Fungus hæmatodes* affects the testicle, as well as most other organs, more frequently in early life. In many cases the disease has appeared to originate from a blow, or other local injury. The termination of the disease has been uniformly fatal; all who have been cut for it have died; our only future hope, therefore, is in an earlier operation.

In the dissection of those who have died in consequence of *fungus hæmatodes*, of some of the external parts already described, tumors of the same medullary structure have been frequently found in some of the internal organs. Examples are accordingly given of *fungus hæmatodes*, or structures similar to this, which were discovered in the liver, in the spleen, and in the kidney.

The soft tubercle or tumor of the lungs, described and delineated by Dr. Baillie, is believed to be referable also to this disease.

An example of *fungus hæmatodes* in the uterus occurred to Mr. Burns; and our author thinks, that a disease of the ovarium described by Dr. Baillie, in which the organ is converted into a pulpy matter, and a similar case he has himself met with, show that the ovary is subject to this change of structure.



In the description which Mr. Hey has given of fungus hæmatodes, three cases are mentioned where the female breast is said to have been affected by it.

"In none of these cases, however," continues Mr. Wardrop, "nor in two others which I carefully dissected, and which appeared to be very analogous to those mentioned by Mr. Hey, was there that medullary structure in any part of the tumor, which has been found to be a constant appearance in fungus hæmatodes in all the other organs where it has been met with. I would therefore be led to doubt the nature of the disease in the cases alluded to, had not fungous tumors arisen from the cicatrix; and being aware at the same time that a very considerable difference may arise in the appearance of this disease in the different organs, the natural structure of the mamma being as unlike that of the sound eyeball or liver, as the same disease in these organs."

This last remark, however we think of no great weight; the eyeball and liver are as unlike in structure as any two parts of the body can be, and yet the medullary structure of fungus hæmatodes is found in both. From the cellular structure of the tumor of the mamma, the interposed transparent glairy fluid found in the cells, the firm membranous intersections found in it, and the want of medullary character, we perfectly agree with Mr. Wardrop, that the propriety of referring this tumor to fungus hæmatodes is very doubtful.

Having thus examined the appearances of fungus hæmatodes in the different parts of the body where it has been found, our author sums up the whole, by exhibiting a comparative view of the structure of fungus hæmatodes and cancer.

Scirrhus is hard, firm, and incompressible, composed of opake fibrous septa, variously arranged, with a softer inorganic semitransparent glutinous substance interposed. The tumor is destroyed by the process of ulceration which ensues, and which consumes in its progress also the neighbouring parts; and the fungus produced from cancer has a firm texture.

"Fungus Hæmatodes presents, on dissection, a very different series of phenomena from the scirrhus tumor. When

it appears in the external parts of the body, and has not yet acquired a considerable bulk, instead of being hard and unyielding, it is soft and elastic, and has an equal surface, giving, in most cases, more or less, a sense of obscure fluctuation. Its form, when taken out of the body, is determined and accurately circumscribed, having generally a distinct covering of condensed cellular membrane. In place of the hard fibrous-looking substance, the principal component part of scirrhus tumors, the morbid growth in fungus hæmatodes consists of a soft pulpy matter, which mixes readily with water, and is hardened by acids, and by boiling in water; and it has been also compared by all who have attempted to describe it to medullary matter, in color and consistence. When the skin or covering of fungus hæmatodes has been eroded by the progress of the disease, instead of the morbid growth being destroyed by ulceration, a fungus arises from it, and the tumor seems only to increase more rapidly in bulk. If the fungus hæmatodes is not interrupted in its progress, both the original tumor, and the fungous mass growing from it, attain a large size; and the fungus, instead of having a firm texture, like that which arises from the cancerous ulcer, is a dark-red and purple colored mass, of an irregular shape, and of a soft texture, is easily torn, and bleeds profusely when slightly injured."

Fungus hæmatodes is found to exist in a greater variety of organs, as in the liver, spleen, kidney, and lungs; organs where the scirrhus structure has never been demonstrated. Those advanced in life are most subject to cancer; fungus hæmatodes is rather to be considered as a disease of early life.

In what particular system, or texture of the organs, does fungus hæmatodes originate?

"It appears certain, that in all those cases in which the disease affected the eyeball, we were ignorant of its existence, before either the optic nerve or retina were changed. In all of the cases, an alteration in the structure of the retina, and an imperfection in the exercise of its functions, were the first symptoms of the disease; and in those cases where the disease advanced farther, no remains of the retina could be detected; and the structure of the optic nerve itself, was changed,



even, in some, as far as the thalamus. In one case, the optic nerve, besides its alteration in structure, was split into different portions by the tumor, which latter seemed to be formed in its external structure. In another case, the medullary portion of the nerve was changed, and the disease extended just to that place where it loses its neurilema, and becomes altogether medullary. In other cases, the neurilema and medullary portions were equally changed."

These observations would lead to a conclusion, "that the disease consists in a morbid change of the nerve itself; and that a growth of medullary matter takes place in this disease, analogous to what is observed in new formations of bony matter, of fat, of skin, or of bloodvessels."

When we reflect on the great variety of organs in which this growth is met with, it appears to us more probable, that the medullary tumor is a morbid matter *sui generis*, which may originate in more structures than one.

But, as our author well observes, "it is extremely difficult to bring forward, on this part of the subject, any thing conclusive. It remains, therefore, an object of most interesting inquiry, if in other parts of the body, which are affected with this disease, besides the optic nerve and retina, any structure of the nerves of the organ can be detected."

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*An account of Spina Bifida, with remarks on a method of treatment proposed by Mr. Abernethy. By Thomas Verney Okes. Cambridge printed; 8vo. 1810. pp. 39.*

From the London Medical and Physical Journal, for October, 1810.

It is the fate of all human invention to have in it the alloy of imperfection. The practice of medicine, and even the art of surgery, which is vaunted to have more of precision and certainty than its congenors, partake of this penalty. The quaint appellative, *noli me tangere*, given to a species of harpers, has been too restricted. The course of actual practice has not unfrequently presented original conformation, and sometimes morbid alteration, which should have affixed to them, *in terrorem, noli me tangere*; of this class is *spina bifida*.

A long acquaintance with the steady judgment and practical skill of Mr. Okes, made us take up his pamphlet with a predilection in its favour. For it is to such men, though, perhaps, unskilled in the art of bookmaking, we are to look for the lessening of human misery, by the improvement of medical science. If in the present instance the experience of this gentleman has enabled him only to give negative instruction, we hope to see him unlock the stores of practical knowledge which many years of application must have accumulated.

The just reward of diligence and eminent abilities is found in that currency of opinions which arises out of reputation. But sometimes has this been injurious, by stamping a value on erroneous doctrines. The deservedly high character of Abernethy induced Mr. Okes to investigate the nature of *spina bifida*, a disease of which Mr. Abernethy had published some cases, and advised a method of treatment from analogy, extremely hazardous, if not always fatal. "From the success with which the evacuation of the matter in lumbar abscess, by means of the trocar, has been attended, Mr. A—— was induced to recommend a similar process in *hydrorhachitis*;" and also to advise pressure upon the punctured sacculus to promote absorption. A number of cases are cited and arguments employed by Mr. Okes to show the dangerous tendency of this practice; and from them are deduced the subsequent corollaries,

1. If an hydrorhactic tumor should ulcerate or slough, so that an opening be made, the patient will inevitably die.

2. If the contents be evacuated by trocar or lancet, and the opening be left unclosed, the patient will die.

3. If a ligature be applied round the base of the tumor, the patient will die.

4. If the contents be evacuated by trocar, and the opening be closed and healed by the first intention, the sac will not contract so as to prevent a fresh accumulation of lymph, but it will be speedily and repeatedly reproduced, and no advantage will be gained by the operation.

The description of *spina bifida*, with which the pamphlet commences, we insert with the view of disseminating practical knowledge.

"The disease which is usually called *spina bifida*, sometimes



divided spine, spinola, and more properly hydrorhachitis, proceeds from a mal-conformation of the spine, and originates with the fœtus in utero. The spinal processes, and sometimes the lateral processes of some of the vertebræ are wanting, by which means there is a longitudinal opening of the bony cylindrical channel which contains the medulla spinalis. This deficiency sometimes takes place in one part of the spine, and sometimes in another; but I believe more commonly in the vertebræ of the loins, or in the os sacrum, than in any other part. It is remarkable that where there is this defect in the formation of the bones, there is generally a corresponding defect of the integuments over that part of the vertebræ; for the cutis and membranæ adiposæ are wanting for a considerable space, and the opening will be found covered only by a very thin, tender skin, and so transparent that the contents may be seen through it; and their covering is doubtless a production of the duramatral coat of the medulla spinalis. Sometimes, however, the common integuments are perfect, and of their natural thickness and opacity. From the difference in the coverings of the tumor it may be distinguished into two sorts, the transparent and opaque. The tumor which is formed contains a clear fluid, like the lymph found in the lateral ventricles of the brain; and hence the disease has acquired the name of hydrorhachitis. There is another shape under which this disease appears, that is when there is no deficiency in the formation of the vertebræ themselves, but where the fluid and the duramatral coat are forced through a small separation of the spinous processes, producing a tumor of the back with a small base. This has given rise to a proposal for what I consider a dangerous mode of practice; for from the smallness of the base of the tumor, practitioners have been induced to suppose that it is easily removable: experience, however, has proved that this cannot be safely attempted."

Cases from Sulpius, Ruysch, and Morgagni, with plates, and observations on the analogy which *spina bifida* has with diseases intimately connected with the coverings of the brain, illustrate and enforce the author's objection to Mr. Abernethy's proposed practice. For several interesting histories of this dis-

ease we refer to the preceding volumes of the Medical and Physical Journal.

Though we have no extravagant liking for the dainty phrases, piquant remarks, and holiday terms of modern authorship, we would be well pleased to see a judicious practitioner sufficiently master of composition, to place his observations in the best light; and we recommend to Mr. Okes, with a sincere regard for his useful talents, that attention to style and arrangement proper to give force and character to his future productions.

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*On the Structure and Uses of the Spleen.*

By EVERARD HOME, F. R. S.

Philosophical Transactions, Part I. 1808.

From the Edinburgh Medical and Surgical Journal, for 1809.

MR. HOME, while investigating the structure of the stomachs of different animals, observed, that in the fourth stomach of ruminating animals, in the single stomach of others, and even in the human stomach, there is a division, more or less evident, between the cardiac and pyloric portions; that while digestion is going on, these portions are separated by means of a permanent or muscular contraction; that the food is prepared, softened, and dissolved in the cardiac portion, and the chyle separated in the pyloric.

In some animals, as in the water rat, the separation between the cardiac and pyloric cavities is very remarkable, and their difference of structure and function abundantly evident; but, in all stomachs, this difference can be traced.

"The first instance in which this muscular contraction was observed in the human stomach, was in a woman who died in consequence of being burnt. She had been unable to take much nourishment for several days previous to her death. The stomach was found empty, and was taken out of the body at a very early period after death. It was carefully inverted to expose its internal surface, and gently distended with air."

"The contraction was so permanent, that after the stomach



had been kept in water for several days, in an inverted state, and at different times distended with air, the appearance was not altogether destroyed. Since that time, I have taken every opportunity of examining the human stomach recently after death, and find, that this contraction, in a greater or less degree, is very generally met with."

"That the food is dissolved in the cardiac portion of the human stomach, is proved by that part only being found digested after death; the instances of which are sufficiently numerous to require no addition being made to them. This could not take place, unless the solvent liquor was deposited there. Mr. Hunter goes so far as to say, in his paper on this subject, 'there are few dead bodies in which the stomach, at its great end, is not, in some degree, digested.' That the chyle is not formed there, and also, that it is completely formed before the food passes through the pylorus, is proved by the result of some experiments of Mr. Hunter's, made upon dogs, in the year 1760; and as they were instituted for a very different purpose, that of determining whether the gastric juice is acid or alkaline, the results were detailed without any possible bias." "In all the dogs, the food was least dissolved, or even mixed toward the great end of the stomach, but became more and more so towards the pylorus; and just within the pylorus, it was mixed with a whitish fluid, like cream, which was also found in the duodenum." "From the result of these experiments," continues Mr. Home, "as well as from the analogy of other animals, it is reasonable to believe, that the glands situated at the termination of the cuticular lining of the œsophagus, which have been described, secrete the solvent liquor, which is occasionally poured on the food, so as to be intimately mixed with it before it is removed from the cardiac portion; and the muscular contraction retains it there till that takes place. Such contraction being occasionally required in the stomach, accounts for its being more or less bent upon itself, which renders it more readily divided into two portions, by the action of the muscular fibres, at that part where the angle is formed."

We have thought it necessary to make these extracts from Mr. Home's papers on the structure of the stomachs of dif-

ferent animals, in order that those of our readers who have not yet had an opportunity of consulting them, might the better understand the account we are to give of his observations on the structure and uses of the spleen. And, for the like reason, we shall subjoin Mr. Home's general conclusions, drawn from the series of the facts and observations detailed in those papers.

"The solvent liquor is secreted from glands of a somewhat similar structure in all animals, but much larger and more conspicuous in some than in others." "These glands are always situated near the orifice of the cavity whose contents are exposed to their secretion.

"The viscid substance found on the internal membrane of all the stomachs, that were examined recently after death, is reduced to that state by a secretion from the whole surface of the stomach which coagulates albumen. This appears to be proved by every part of the fourth cavity of the calf's stomach having the property of coagulating milk.

"This property, in the general secretion of the stomach, leads to an opinion, that the coagulation of fluid substances is necessary for their being acted on by the solvent liquor; and a practical observation of the late Mr. Hunter, that weak stomachs can only digest solid food, is in confirmation of it.

"In converting animal and vegetable substances into chyle, the food is first intimately mixed with the general secretions of the stomach; and, after it has been acted on by them, the solvent liquor is poured upon it, by which the nutritious part is dissolved. This solution is afterwards conveyed into the pyloric portion, where it is mixed with the secretions peculiar to that cavity, and converted into chyle." *See Philos. Transac. part 2d, 1807.*

Believing, then, that the cardiac and pyloric portions of the stomach are separated from each other by a permanent or muscular contraction, and that they perform different and distinct functions during digestion, Mr. Home, in the paper now before us, endeavours to show, that there is between the cardiac portion of the stomach, and the circulation of the blood, a communication through the medium of the spleen; and that



the use of this organ is to carry off the fluids from the cardiac portion.

First, He observes that the fluids are chiefly found in the cardiac portion, while the contents of the pyloric are commonly of uniform consistence. It seemed, therefore, probable, that the fluids beyond what are necessary for digestion are carried out of the stomach, without ever reaching so far as the pylorus.

Secondly, He made the following experiment. The pylorus of a dog was firmly secured by ligature, and five ounces of water, colored with indigo, were injected into the stomach; at the end of half an hour, two ounces of nearly colorless fluid were rejected by vomiting. The dog was now killed. Though the pylorus was completely closed by the ligature, the pyloric portion of the stomach was empty and contracted. "The cardiac portion contained about two ounces of solid contents, enveloped in a gelatinous substance, and one ounce of water with little or no color, the indigo being completely separated from it, and spread over the surface of the internal membrane."

Two ounces of fluid had therefore escaped from the stomach in half an hour. No absorbents could be seen; but "the spleen was turgid, unusually large, and its external surface very irregular. When cut into, small cells were every where met with, containing a watery fluid, and occupying a considerable portion of its surface."

These cells he finds to be the glands of Malpighi, and corpuscles of Cuvier. These cells are only distinct, and seen to contain a fluid, when examined immediately after the stomach had received an unusual quantity of liquids. The fluid escapes when the cells are punctured. The cells are membranous, and have numerous arterial branches and plexus of veins ramifying on their coats.

The trunk of the splenic vein compared with that of the artery, Mr. Home found to be in point of size as 5 to 1.

Mr. Home repeated his first experiment, making use of an infusion of madder, instead of a solution of indigo, in hopes that the coloring matter might be detected in the spleen. The result, however, did not correspond to his expectation.

At last, on the suggestion of Mr. Brande, he made use of

rhubarb, which speedily passes into the blood, and tinges the urine very soon after being received into the stomach, and very small portions of which can be detected by means of the caustic alkali, used as a test. "Five drops of tincture of rhubarb, added to three ounces of water, are found to strike an orange tint, when the test is added."

By means of this test, the rhubarb was detected in the urine of different persons, in seventeen and twenty minutes after the tincture had been swallowed. These circumstances being ascertained, Mr. Home performed the following experiment.

"On November 17th, 1807, at thirty-five minutes past eleven o'clock, five drachms of a mixture of tincture of rhubarb and water, in the proportion of a drachm to an ounce, were injected into the stomach of a dog, whose pylorus was secured. At twenty minutes past one, two ounces of fluid were brought up by vomiting; ten minutes afterwards, another ounce of the mixture was injected, as were nine drachms more at half past four o'clock.

"The two last portions were retained, and at eight o'clock in the evening the dog was killed.

"On examining the parts after death, the pylorus was found to be completely secured; the stomach contained about two ounces of fluid; none of the absorbent vessels passing from its great curvature were in a distended state, so as to be rendered visible. The spleen was turgid as in the former experiment, and the urinary bladder full of urine.

"The urine, tested by the alkali, received a deeper tinge of rhubarb than the human urine, after rhubarb had been taken three hours by the mouth, and in other respects resembled it.

"When the spleen was cut into, the cells were particularly large and distinct. A portion of it was then macerated in two drachms of water, for ten minutes in a glass phial. All the parts were exposed to the water by its being divided in all directions. The water thus impregnated, was strained off and tested by the alkali, and immediately the redish brown color was produced in the centre, and no where else; but in less than a minute it began to diffuse itself, and extended over the whole.

"A similar portion of the liver was treated in the same way,



and the alkali was added to the strained liquor; but no change took place in it whatever. In this experiment, the rhubarb was detected in the juices of the spleen, as well as in the urine; and as there was no appearance of it in the liver, it could not have arrived there through the medium of the common absorbents, carrying it into the thoracic duct, and afterwards into the circulation of the blood."

Mr. Home here concludes, observing, "The discovery of this fact I consider to be of sufficient importance to be announced to the society, that when it is thus made public, I may be at liberty more openly, and on a more extensive scale of experiment, to prosecute the inquiry."

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*A Dissertation on Retroversion of the Womb, including some Observations on Extra-Uterine Gestation. By Samuel Merriman, M. D. 8vo. Lond. 1810. pp. viii—80.*

From the London Medical and Physical Journal, for November, 1810.

THE substance of this dissertation originally appeared in the volumes of this journal. In its present dilated and improved form, it is intended to establish two points of importance in the theory of pregnancy, and the practice of obstetrics.

Retroversion of the uterus, as a fact occurring at one period of uterine gestation, is well known; the extension of this displaced position of the womb to the last term of pregnancy is not, however, so fully ascertained; neither has it hitherto been subjected to much investigation. Dr. Merriman adduces several cases to prove the existence of this state of the uterus in the latter months of gestation, and that it also gives rise to another fact in the history of pregnancy, commonly denominated *extra-uterine gestation*; but which has, from its discordance with established physiological principles, remained doubtful, obscure, or "incomprehensible."

That retroversion of the uterus must have occurred at all periods cannot be doubted; but that its real phenomena have only lately been understood occasions more regret than surprise. Neither Mauriceau, Lamotte, nor Deventer, who attended particularly to a state of the womb, which we denomi-

nated its obliquity,\* had any precise knowledge of this altered position of the uterus, nor of the effect it produced on the bladder and rectum; deranging the functions, and obstructing the operations of these organs. "The first person, who entertained any correct idea of this accident, was M. Gregoire, an accoucheur of considerable reputation at Paris, about the middle of the last century, and a lecturer on the practice of midwifery. M. Gregoire's lectures were attended by many students, who afterward became celebrated accoucheurs; and among others our countryman *Smellie*: yet his doctrine upon this subject seems to have been recollected by only two of his pupils,—the French accoucheur M. *Levret*, and Mr. *Wall*; and from the latter the accoucheurs of England first gained information on this subject."

"The lectures which Gregoire delivered were never printed; and therefore it is impossible to know exactly what opinion he had formed of this accident; but that he was aware, of the *fundus uteri* being thrown down between the vagina and rectum, is evident from the directions which he gave for replacing it†. In the year 1754, Mr. Wall, who had settled in London, was called to attend a woman labouring under a suppression of urine, from a retroversion of the uterus; and being convinced that it was a case of the kind described by Gregoire, he endeavoured to relieve his patient by the method taught by that lecturer; but failing in his object, he called in the assistance of the late Dr. William Hunter. Their united endeavours in various ways were unavailing, and the woman died! Dr. Hunter, with the most laudable desire to disseminate knowledge, having obtained leave to open the body, invited a number of gentlemen of the faculty of medicine to a public lecture, which he read for the purpose of making the disease generally known. Drawings were taken

\* *Rien ne lui fit plus d'honneur, says his biographer, que d'avoir prouvé que l'obliquité de la matrice est une des premières causes des accouchemens difficiles, & d'avoir indiqué la manœuvre que demandent les accouchemens de cette espece.*

† M. Gregoire instructed his pupils, that with one finger in the vagina and another in the rectum, an attempt should be made to raise the fundus and to replace the uterus. This was advised to be done while the woman was lying on her back, an unfavourable position for the purpose: for, should this manœuvre ever be deemed necessary, the objects of it may be accomplished with more facility while the patient is resting on her elbows and knees.



"of the parts, from the dissection of this woman, and an engraving from these was inserted in Dr. Hunter's splendid *"Anatomy of the Human Gravid Uterus."* This case, however, did not come fully before the public, until, in 1771 it was printed in the 4th volume of the Medical Observations and Inquiries (p. 400), as an appendix to a history of retroverted uterus, there related by Mr. Lynn, of Woodbridge in Suffolk. Mr. Lynn's narrative is, we apprehend, the first printed account of this form of retroverted uterus published in England.

From the year 1771 this accident has so often been described, that its symptoms are perfectly understood, and are relieved with great certainty by the simple process of introducing the catheter so frequently as to prevent the bladder being distended with urine.† For many useful observations on this form of

† In the cure of retroverted uterus, the principal reliance is to be placed upon the introduction of the catheter; and this should be done twice at least in twenty-four hours. Care is likewise to be taken to keep the bowels open, and rest is to be enjoined. By pursuing this plan steadily, the mal-position of the uterus is usually overcome in a few days. It is still customary with some practitioners of eminence to make use of artificial means for replacing the womb, after the bladder has been emptied and the bowels opened; and there can be no great objection to making such an attempt, provided it be done cautiously, and that no (improper) force be made use of. In general, however, nothing of this kind is either necessary or advisable. p. 24.

In many cases of retroverted uterus which have fallen under our care, the catheter thus employed, enjoining a recumbent posture, and attending to the state of the bowels, has been completely successful. In some of these cases the uterus was alarmingly impacted in the pelvis; yet the termination was always fortunate. The introduction of the catheter, and the emptying the bladder by that means, being the desideratum in the management of this complaint, the following practical observations by Dr. Denman cannot fail to be acceptable "It cannot be disputed (*Introduction*, vol. i. 140.) but that all attempts to restore the uterus to its natural position, before the distention of the bladder is removed, must be fruitless, as the uterus will be irresistibly borne down by the pressure of the superincumbent bladder. The first step then to be taken for the relief of the patient is, to draw off the urine; yet there is always in these cases great difficulty in the introduction of the common catheter, because the urethra is elongated, altered in its direction, and pressed against the ossa pubis by the tumor formed by the retroverted uterus; and many women, when the uterus was retroverted, have lost their lives from the want of expertness in introducing the catheter. But the attending inconveniences may be avoided or surmounted by the use of a flexible catheter, slowly conducted through the urethra. I say slowly, because, whatever catheter is used, the success of the operation, and the ease and safety of the patient, very much depend on this circumstance. The catheter, when introduced, should not be carried farther into the bladder, when the urine begins to flow, unless it ceases before the distention is removed; which in some cases happens in such a manner, as to give us an idea of a bladder divided into two cavities."

the disease, we are compelled, though unwillingly, to refer to the dissertation itself; and can only remark, that retroversion has also recurred in the unimpregnated state of the uterus, when it has been enlarged by disease. Of this an instance is related by Dr. Merriman; and Mr. John Pearson (*Observations on Cancerous Complaints*, 8vo. Lond. 1723), on examination, in a case of diseased uterus, "found it hard, immovable, and "so much enlarged, that it filled up the superior part of the "vagina: it was also retroverted, the *os uteri* pressing against "the bladder, and the *fundus uteri* compressing the rectum." p. 114. In these cases, however, it is obvious that the danger, as arising out of the position of the uterus, will neither be so great, or approach with such rapid strides, as in the impregnated state of that organ.

The continuance of the impregnated uterus in a retroverted position to the last period of utero-gestation, is a fact possessing both novelty\* and interest; especially as Dr. Merriman deduces conclusions from it, explanatory of what has been deemed *extra-uterine gestation*.

Of this state of the uterus Dr. Merriman relates two instances; one of them is that which occurred to Dr. Jackson, as specified in the note below, and the other is a minutely circumstantial history of a case which happened in 1806. It is hardly to be doubted, however, that this variety of retroverted uterus, differs very essentially from that which occurs in the early period of pregnancy. In the malposition of the uterus, prior to the rising of that organ above the brim of the pelvis, the danger is hourly increasing by the increasing size of the uterus while impacted in that bony cavity; and by which its pressure, both on the bladder and rectum, is carried to a point that completely obstructs the expulsion of their contents. In the other variety, that which continues to the full term of utero-gestation, the womb being necessarily above the brim of the pelvis, and lying among parts that may recede from its pressure, though much inconvenience will arise, no urgent danger presents till the period of parturition. We mistake

\* We have much satisfaction in stating, that our friend Dr. H. J. Jackson, of Hanover-street, in a judicious little work, (*Cautions to women respecting the state of pregnancy*, &c. 8vo. Lond. 1798,) published the first case of this disease.



Dr. Merriman exceedingly, if he does not lead his readers to understand that this second variety, or that which lasts to the end of utero-gestation, is a continuance of the first variety, or the retroverted uterus of early pregnancy; and that it is one of its terminations.\* "It has been," he says, "the almost uniform opinion of writers on the retroversion of the gravid uterus, that a suppression of urine must necessarily be produced by this unnatural situation of the womb, and that this suppression must be removed by art; otherwise, they say, abortion will take place, or a still more melancholy event, mortification, or rupture of the bladder, and consequently the death of the patient will ensue; and, in cases where suppression of urine has come on, this has unquestionably *been the usual progress* and termination of the complaint, unless proper means of cure have been had recourse to at the commencement of the disease. It is, however, consolatory to know, that, under some circumstances, *the uterus may remain in a state of retroversion* for a very great length of time, even to the completion of the period of utero-gestation, without producing a total suppression of urine, or any other very uncommon or alarming symptoms." (p. 25.) It is probable, however, and it is a fact of some importance both as to the true history of the disease and to the practice, that these are two distinct complaints; one being necessarily confined to the first period of utero-gestation, that is, before the uterus rises above the pelvis; the other, as necessarily occurring in that period when, in its enlarged state, the womb has risen into the cavity of the abdomen: one being accompanied with immediate and urgent danger, the other with injurious consequences remote and uncertain. It does not appear that those who have suffered under that variety of retroverted uterus, which continues to the time of parturition, have experienced the symptoms and the haz-

\* At page 76, Dr. Merriman puts this past all doubt, for he there positively says, "that the uterus, which has become retroverted in the earlier stages of pregnancy, *may continue in that state* till the full period of gestation has elapsed." It is not designed to contend that the two varieties of retroversion may not exist at different periods in the same pregnancy; but if they do exist, that the coincidence is accidental, and that they have no dependence one on the other. In Mrs. F——'s case there is no ground to presume an early retroversion. In the case related by Mr. Kelson there was an early retroversion, but this was completely removed long before the accession of the other disease.

ards of that which appears in early pregnancy. "Mrs. F—  
 "did not suffer more, during her pregnancy, than most other  
 "women, except that for the *last two or three months*, she was  
 "troubled with difficulty in parting with her urine, and con-  
 "siderable pain in the act of passing it; yet her sufferings in  
 "this particular were not so great as to induce her to consult  
 "her accoucheur on the subject. She neither at this time, nor  
 "at any earlier period of her pregnancy, experienced a total  
 "suppression of urine, nor does she recollect ever having re-  
 "tained it long enough to occasion any considerable incon-  
 "venience." (p. 28.) In Mrs. F—'s case there is a distinct  
 point of time marked for the commencement of her disease.  
 "When about *five months* advanced in her pregnancy, she was  
 "much terrified and affected on hearing of the sudden death of  
 "her aunt; which, as she herself expressed it, *seemed to turn*  
 "*her whole inside upside down.*" Whether the feelings of  
 Mrs. F— were, or were not, sufficient to determine that  
 then the altered position of the uterus took place, does not  
 seem of much weight; for it is evident, from the history, that  
 it had not been preceded by the unvarying symptoms of the  
 first variety of retroverted uterus. In the case related from  
*Nicholas Patuna*, the patient, "about six weeks after concep-  
 "tion, was attacked with violent pains in the lower belly, and  
 "was unable to pass her urine, except when lying on her back.  
 "From this time, till she entered the ninth month of her preg-  
 "nancy she suffered much at various times, from pains in the  
 "belly, and loins, and other complaints." These symptoms,  
 though they show the existence of some morbid change, are  
 certainly no evidence that this change was the first variety of  
 retroverted uterus. Mr. Kelson's case, related in the 11th  
 volume of this journal, was, in the first instance, unequivocally  
 the retroverted uterus of early pregnancy. It was cured by  
 the usual method (a daily introduction of the catheter); and  
 about the 12th week, the impediment to the flow of urine being  
 removed, *the uterus took pretty much its natural situation.*  
 (p. 52.) After this, it appears that the second variety of re-  
 troversion took place; but at what period was never determin-  
 ed; nor was it demonstrated that this wrong position of the  
 womb did exist, until after the usual period of gestation had



elapsed, when the indications of labour enabled Mr. Kelson to ascertain "that the child was plainly to be felt through the vagina, that the uterus was not enlarged, but forced upwards "and forward, and the *os tinæ quite closed*."

Under every point in which we have viewed this statement, a conviction has arisen, that the variety of retroverted uterus, occurring in the latter months of pregnancy, has no dependence on, or connexion with that which appears in the early period; that is, before the fourth month of utero-gestation. With this impression, we regret that the disease which Dr. Merriman has so accurately traced, and from which he has deduced a conclusion greatly interesting to science, should have received the name of *retroverted uterus*. This we regret because it wants precision—because it confuses it with a disease already well known, and with which, we believe, it has no connexion—and because it may, in some instances of the true retroverted uterus, lead to a fatal procrastination.

We entreat that Dr. Merriman will consider that these observations are not made with the intent to depreciate his ingenious dissertation, but in the spirit of scientific investigation, and with the ardent desire to elicit truth.

To whatever degree we may dissent from the opinion, that the deranged position of the womb sometimes met with in the latter months of pregnancy, is a continuation or termination of a case, so fully known under the term *RETROVERSION*:—or how much soever we may object to the application of the same name to two distinct diseases, we readily admit that the subject has been greatly elucidated by our author; and we believe the following conclusions to be formed on the solid ground of fact and fair reasoning.

Whenever that peculiar state of the uterus which Dr. Merriman denominates *retroversion*, and which he describes as a continuance of the retroversion of early pregnancy does take place, it may be expected to terminate in one of the following ways.

1st. "The good form of the pelvis, the health and strength of the mother, and the efficacy and continuance of the pains, may all combine to replace the uterus, and produce a favourable issue.

2dly. "The want of some, or all, of these fortunate circumstances, or injudicious management under the labour, may occasion the woman to fall a victim to this untoward position of the womb, in the course of a few days, either by a rupture of the uterus producing speedy death, or by an active inflammation or mortification of the parts.

3dly. "The uterus being unable to extricate itself out of the awkward position into which it is thrown, may passively submit to the burthen, till by the slow process of ulceration, the fœtus may be excluded through the rectum or vagina, and the mother remain alive."

In the preceding part of the dissertation, the observations and facts have been principally employed in elucidating that favourable termination of the disease, which occurs when, from the good form of the pelvis and other circumstances in the patient, the womb is restored to its natural position, and the fœtus is extruded by the proper uterine action. The remaining part enters on a disquisition extremely curious and interesting.

The frequent recurrence in the records of medicine of histories of *extra-uterine gestation* would, it might be presumed, have rendered the fact, great as the violation of a law of nature is, sufficiently distinct and tangible. The investigation to which Dr. Merriman has subjected these histories, has demonstrated, however, that some of them must come under a very different class; while over the remainder is spread a degree of doubt that renders their character equivocal.

"Mr. Kelson, Mr. Colman, and Mr. Mainwaring, have all denominated their communications,\* cases of *extra-uterine fœtus*. If by this is merely meant that the fœtus, after being brought to maturity, or nearly so, in the uterus, was excluded from thence into the cavity of the abdomen, either by a rupture of the uterus, or by an ulcerated opening through its parietes; and afterwards, by the same process, discharged through the rectum or vagina, I completely accord in the same opinion; but if, as I understand, it is supposed that these fœtuses were actually nourished, and brought to per-

\* Cases given in Dr. Merriman's dissertation from the Medical and Physical Journal, (vols. 2 and 11,) and from the 2d volume of the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge.



“section in the cavity of the abdomen, unconnected with, and  
 “perfectly distinct from, that system of vessels which have  
 “been expressly set apart by nature for this purpose, I must  
 “confess myself not at all convinced by these cases of the ex-  
 “istence of a fact, so completely at variance with all we know  
 “of the laws of nature, and of the animal economy. I mean  
 “not to deny,” he adds, “that fœtuses have been found in the  
 “cavity of the abdomen, entirely disengaged from the uterus  
 “and its appendages; but I think that in such instances a *rup-*  
 “*ture* of the womb, of the tube, or ovarium, or an ulceration  
 “through their parietes, had permitted the escape of a full  
 “grown fœtus, and not that the conception had advanced to ma-  
 “turity in a part apparently so ill adapted to such a purpose.  
 “Every one of the instances of *extra-uterine gestation* which I  
 “have had an opportunity of perusing, has been deficient in  
 “some of the particulars, or symptoms, which are necessary  
 “for confirming the fact, of a fœtus being nourished in a cavity,  
 “and by vessels intended for other purposes, and distinct from  
 “the uterine system; and at the same time the case was capable  
 “of being explained in a more rational way.”

Dr. Merriman examines, with much acumen, several cases which have been considered by their authors as extra-uterine; and shows, with great force of probability, that they have arisen from a rupture or ulceration of the uterus, consequent to that altered position which he has denominated *retroversion*. We are able to add to the catalogue a well marked history, where the fœtus escaped from the tube, passed by the medium of ulceration into the rectum, and was expelled, *per anum*; and where the uterus had also taken the position which is considered to give rise to such cases. It occurred to Mr. Giffard the surgeon, and was published in 1730, in the *Phil. Trans.* vol. 36. p. 435.

“I was sent for about the middle of August,” says Mr. Giffard, “to a woman who then judged herself to be between  
 “three and four months gone with child: she had all the symp-  
 “toms preceding a miscarriage, and upon touching I found the  
 “*os tinæ* somewhat dilated and spread, and from whence I  
 “concluded miscarriage would ensue; but I was some time  
 “after informed, that although she before believed that she had

“miscarried, that she now thought herself quick; as feeling  
 “somewhat to move within her, agreeable to what she had per-  
 “ceived after former quickenings. Thus it passed on for about  
 “six or seven weeks; in which time she grew much bigger,  
 “and the motion more perceptible; so that there remained no  
 “doubt of her being with child. On the third of October she  
 “was seized with violent pains in her belly and back; which  
 “daily increasing, I saw her on the sixth, and found her la-  
 “bouring under very great pains, and other complaints like  
 “those preceding miscarriage or delivery. To be better satis-  
 “fied, I passed up two fingers into the vagina, to examine by  
 “the touch, whether the *os tincae* began to open. *I there felt*  
 “*a large and unusual fulness and tension, which I then judged*  
 “*to be the body of the uterus sunk low in the vagina, and much*  
 “*distending it, and extending backwards, and pressing against*  
 “*the rectum, so that the excrements could not pass, neither*  
 “*could she, from its pressure upon the neck of the bladder, freely*  
 “*make water. I could not find the os tincae, although I very*  
 “*carefully examined; wherefore I then judged that the fundus*  
 “*uteri must have receded from its natural position, and be bent*  
 “*backward toward the rectum: in which opinion I was the*  
 “*more strengthened, from the fulness I had observed stretching*  
 “*backward; and therefore concluded that the os tincae must be*  
 “*very forward: wherefore I endeavoured to pass my fingers*  
 “*between the os pubis and the fulness which pressed against the*  
 “*upper edge of the said bone. This with some difficulty I effect-*  
 “*ed, and at length, about two or three inches above the said*  
 “*bone, I felt the os tincae with the ends of my fingers. The*  
 “cause of this situation will more clearly appear in the pursuit  
 “of this account. I ordered her anodyne and quieting medicines  
 “to relieve her pain, which she was obliged to repeat every  
 “twelve hours; and sometimes clysters. Thus matters continu-  
 “ed to the 20th of the said month, only that for some days  
 “before, a water tinged with blood came away, as she imagin-  
 “ed, through the anus.

“On the 20th her husband came to me, with an account that  
 “the midwife had brought away a fœtus, but could not com-  
 “plete her business. The midwife informed me that a fœtus  
 “was protruded through the anus; and to confirm it, desired



“me to examine, which I did, and found the *funis umbilicalis* hanging out about three inches beyond the anus, and passing up through the same. I passed my fingers by the string into the anus; where I found, about three inches up, an opening as I then judged, into the uterus, wide enough to admit the ends of three or four fingers, and the *funis* passing into it; from whence I was assured that the fœtus came out that way. The septum between the anus and vagina was entirely whole. From these appearances I then concluded that a mortification must have begun in the uterus; and, from its contiguity, been communicated to the rectum; so that nature, endeavouring to expel what was contained, and forcing it against this part already mortified, produced this opening, and the protrusion of the fœtus through it into the *rectum*.

“There was a large discharge of grumous blood and other substances through the anus, which continued until the 26th, when the woman died. The fœtus was perfect in all its parts, but wasted from its being some time dead.”

Mr. Nourse, then one of the Surgeons of Bartholomew Hospital, examined the body, and has given an explicit dissection, with drawings of the morbid parts.

The *vagina*, *uterus*, *ligamenta rotunda*, *left ovarium*, *fallopian tube*, and *ligamentum latum* on that side, together with the hypogastric and spermatic vessels of the same side, were in a natural state. The *fallopian tube* on the right side, being traced from the *fundus uteri* almost to the *morsus diaboli*, was found confusedly uniting with, and opening into the *sacculus*, hereafter to be described. The *ovarium* on this side, with the *ligamentum latum*, was dilated into a large *sacculus*\* of an irregular form, extending behind the *uterus*, to the posterior parietes of which it adhered, and passing on toward the left, was connected to that part of the *colon* that terminates in the *rectum*, and to the *rectum* itself.

In all its circumstances this case has considerable precision; the exact nature of it is verified by dissection, and the annex-

\* In this *sacculus* was contained the fœtus, and on the dissection were found in it the placenta and several lacerated membranes; and from it there was a large opening into the *rectum*.

ed plate, in the volume referred to, renders the whole history very clear.

We cannot close our view of this valuable "Dissertation" better, perhaps, than by quoting from it the practical remarks which the author makes on the management of those heretofore mysterious and mistaken cases.

"Dr. Hall, in his elaborate defence of the Cæsarean operation, considering all those cases as extra-uterine, recommends to cut through the back part of the vagina, and to bring the child through the os externum; and the same operation has been recommended by Dr. Mackenzie and Dr. Kelly. There is a case quoted by Dr. Hall, which happened to M. Lauverjat, in which the child was extracted by an incision into the uterus by the vagina. There is a great want of accuracy in describing the situation of the womb; but I can only conceive of it, that it was in a retroverted state; and that the incision was made through the posterior part of the vagina into the uterus. Unless this were the position, I see no reason that there could be for operating at all. Une femme enceinte pour la première fois, et parvenue au moment d'accoucher, éprouvoit des douleurs si vives, que Lauverjat voulut s'assurer de l'état des choses. Il fut surpris de trouver la vulve occupée par un corps qui la remplissoit, et la depassoit, et qui cédoit à l'impulsion des doigts, excepté dans le tems des douleurs. En parcourant cette tumeur, il ne trouva à la circonference qu'un cul-de-sac de demipouce de profondeur, sans ouverture, qui peut permettre la sortie de l'enfant. Des confères mandés pour ce cas extraordinaire, voulurent voir aussi comment les choses se passoient. Ils trouvèrent sur la tumeur, une déchireure qui intéressoit q'une partie de l'épaisseur de ses parois. Cette déchireure leur parut le lieu, où il falloit inciser. L'opération faite, le doigt entra dans la poche dans laquelle l'enfant étoit contenu. Il sortit beaucoup d'eau bourbeuse. L'enfant se presenta et franchit l'ouverture, qui venoit d'être pratiquée, et à laquelle il se fit une petite dilaceration du côté droit. Lauverjat ayant porté la main dans la poche, ne trouva aucune trace de col ni d'orifice. Du reste il ne survint point d'accident, et les écoulemens se firent à travers l'ouverture,



“ qui se ferma par degrés. Deux mois après, le col et l’orifice  
 “ de la matrice étoient dans leur état naturel. (*Sabatier de la*  
 “ *Medicine Operatoire*, tom. i. p. 816.) The event of this case  
 “ would, doubtless, under some circumstances, justify an ope-  
 “ ration; but considering the possibility, not to say probability,  
 “ of all such cases being retroversions of the uterus, and  
 “ having the knowledge of two which terminated so favoura-  
 “ bly from the sole efforts of nature, as Mrs. Walker and  
 “ Mrs. F——s, it is impossible that I can recommend an ope-  
 “ ration, especially at the commencement of the pains, and  
 “ while the strength, habit of body, and general health of the  
 “ patient are uninjured. So long as these circumstances re-  
 “ main propitious, there is great reason to expect that the  
 “ pains alone may be able to rectify what is amiss, and to  
 “ effect the delivery *per vias naturales*. The observation of  
 “ Cicero, *Naturæ solertiam, nulla ars, nulla manus, nemo opifex*  
 “ *consequi potest imitando*, should, as far as possible, be our  
 “ guide in the practice of midwifery; in which we find that  
 “ women are seldom injured by a long continuance of labour  
 “ pains, provided their spirits are not hurried, nor their blood  
 “ inflamed, by injudicious attempts to give assistance, and by  
 “ the improper use of cordials and stimulants.

“ If, however, nature gives up the point, if it is found that  
 “ the pains are unequal to the task of restoring the uterus to  
 “ its place, and accomplishing the delivery of the woman, it  
 “ remains to be considered, whether it is not more to the ad-  
 “ vantage of the patient, that such an operation should be per-  
 “ formed, and that she should by this means be released from  
 “ her burthen, than that she should continue in the deplorable  
 “ condition, which attends the slow process of discharging  
 “ a fœtus through an ulcerated opening in the vagina or rec-  
 “ tum; in which process there must always be great pain and  
 “ suffering for many months; and in which, though several  
 “ women have passed happily through it, yet others have  
 “ perished miserably, exhausted with pain and debility.

“ A justification of such an operation may doubtless be  
 “ found, and perhaps it may, in some rare instances, be ad-  
 “ visable; but we know, from woful experience, that inci-  
 “ sions into the uterus have been so very generally fatal, (in

"this country in particular) that the experiment ought never  
"to be tried, while any reasonable expectation can be indulged  
"of saving the patient by any other means."

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*Observations on some of the Principal Diseases of the Rectum and Anus, particularly Stricture of the Rectum, the Hemorrhoidal Excrescence, and the Fistula in Ano. By Thomas Copeland, Fellow of the College of Surgeons, and Assistant Surgeon to the Westminster General Dispensary, 8vo. London, 1810. pp. 100. Callow.*

From the Medical and Physical Journal, for November 1810.

MR. COPELAND opens this very interesting subject on the Art and Practice of Surgery, with a sensible and modest preface, introducing the observations in an order perspicuous and methodical, without calling away the attention by any inducement from the valuable information given in the whole of this work. The contents are useful, true and important. They will furnish the practitioner with such arguments for treating the disease, of which this pamphlet is the object, that the impression derived from it must give a strong confidence in practice to him who is fortunate enough to study, digest, and embrace it. Whatever is advanced by Mr. Copeland, the reader will find supported by other authors; and whatever is opposed, will be seen to be a difference in opinion, by which the reader has the choice of forming his own judgment. The selections are arranged in the very manner of the most respectable of the ancient writers, either to corroborate the author's own sentiment, or to show his variation from their text. We hail the time, and congratulate surgeons upon the event, that in this pamphlet we see not only ancient, but living authors, quoted with all becoming respect; we see also in this work the scholar of industry, to whom the languages of the different authors he has referred to are open to his research.

Mr. Copeland has divided this important work into sections. The first of which is, "*Observations on the Stricture of the Rectum.*"

The rectum is here considered by Mr. Copeland, like any



other part, subject to the consequences of inflammation, and the case aggravated from the action which that intestine is every day compelled to perform in the expulsion of the fæces. He supposes that obstinate cases of Ileus, and other chronic bowel complaints which resist treatment, have their origin in organic obstruction of the canal of the large intestines. He cites, in confirmation of this, authorities of the highest fame. He proceeds to state that strictures are not usually detected early in these complaints, and frequently not discovered till after death: hence, they have been deemed to be fatal, though the fatality was in the want of discovering them. After Mr. Copeland has given us the benefit of the opinions of most ancient authors, and after he has paid that high tribute to Ruysch which we forbear to quote, he proceeds to inform us of that of the moderns. Dr. Sherwin and Dessault are intitled to high notice for what they have imparted on this subject. And when he has thus disposed of the authors to whom he has referred, he concludes with this summary.

“But though those who are afflicted with this disease are sometimes quickly carried off with symptoms resembling those of ileus, this is not the most usual form of the complaint, for it commonly assumes a more chronic character. It attacks people of both sexes, and of almost all ages; but is most common about the middle age, and I think, as far as my experience goes, that women are more frequently affected than men.\* The first symptom of the disease is an habitual costiveness; but this is so frequent an occurrence, and produced in so many ways, that it is not likely that the cause should be sought for in an organic affection of the rectum; mild purgatives are resorted to, and the symptom being relieved, the cause is no longer sought after.

\* The cases which Mons. Derregagnix relates in Dessault's journal, are principally women, and the most aged is only forty-six years old. Mr. White, in a paper in the London Medical and Physical Journal, Oct. 1809, remarks, that this disease is most common at the decline of life; but his own experience does not bear him out in the assertion, for of eight cases which he gives, six are under fifty years of age. Mr. White considers the disease as necessarily fatal, but he does not seem to have heard of Mons. Dessault's paper.

"When this has subsisted for some time the patient complains of what is called piles, and what is often really so, as a consequence of obstructed circulation in the parts. The remedies usually given in such cases are applied, sometimes with relief, but more frequently otherwise; and then the good old maxim of the inexpediency of curing piles, perhaps rescues the practitioner from the discredit of failing to relieve his patient. The piles are sometimes removed by ligature, or excision, and this gives a temporary abatement of the most painful symptoms, while the cause of the disease is still unknown.

"In a short time, as the gut continues to decrease in diameter, the efforts to expel the *fæces* become more violent, and the consequent progress of the disease more rapid. The stools, which have been long evacuated with difficulty, become contracted in size, appearing like earth-worms in their form, or small pellets. In this stage it is sometimes, in the male, mistaken for enlarged prostate gland, but if the finger be introduced into the rectum, the gut will be found either obstructed with small tubercles;\* or intersected with membranous filaments;† or else the introduction of the finger will be opposed by a hard ring of a cartilaginous feel, composed of the diseased inner membrane of the intestine,‡ instead of that regular tumor on the anterior part of the rectum, which is formed by an enlargement of the prostate gland. As the disease advances, the *fæces* become more fluid, and there is a thin sanious discharge from the anus, accompanied with tenesmus; not however the painful tenesmus of dysentery,§ but with less distress and less irritation of the parts than in that disease.

"During this time the constitution suffers so little, that the patient might be supposed, from appearance, to enjoy full health. But the ravages of the disease now begin to be felt in their effect on the general habit. Frequent eructations of air

\* Dessault loc. citat.

† Bonetus loc. citat. Case 5 in this book.

‡ Morgagni, annulo quasi quodam constringi videretur digitus, loc. citato. Adeo induratum ut anceps harerem an carnosum an cartilagosum esset dicendum. Ruysch, t. iv. obs. 95.

§ Sherwin.



confined in the intestines, added to the other symptoms, torment the patient, and render his life miserable. This symptom is so constant, that if it did not occur also in affections of the kidneys and other complaints, it might be regarded as pathognomonic; but I think it prevails to a greater degree in this than in any other disease.

" At this period abscesses very frequently form in the neighbourhood of the anus, and sometimes break into the vagina in the female, and the fæces are discharged through the fistulous orifice. In the male, an adhesion takes place with the bladder, and the abscess\* discharges itself with the urine, and sometimes fæces and wind are voided by the urethra. But more frequently the matter makes its way through the nates, as in cases of common fistula, for which disease it is not unfrequently treated.

" The patient often continues a long time in this distressing situation, for none of the vital organs are affected, till, at last, worn out with the pain and the discharge, or perhaps the total obliteration of the rectum, he yields to his fate. This is usually the progress and issue of the disease, when it is not early discovered, and I must confess also, sometimes the termination when it is; that is, when the parts are attacked with cancerous ulceration. But I believe that, when the cause of the complaint is ascertained in its early stage, the resources of the healing art are sufficient very materially to relieve, and often to cure it altogether; subject, however, like strictures in the urethra, to the necessity of now and then passing a bougie for a considerable time after the symptoms are removed.

" † The rapid progress of disease, from a very trivial origin, in parts subject to continual motion, is very remarkable, in many instances, besides that one under consideration; for this reason, a simple wound near the organs of deglutition is

\* Petit Œuvres Posthum. tom. ii. p. 93, the contrary also occasionally takes place, and urinary calculi are voided by the rectum. See Paulus Ægineta, and Memoirs of the Medical Society, vol. iii. 496, 542.

† See some very important observations on this subject in Mons. David's Memoir on the Effects of Motion and Rest, translated by Justamond. Also, Mons. Bazille sur les Effets des contre Coups en divers parties du Corps. Prix de l'Académie de Chirurgie, tom. iv. quarto.

so difficult to heal; for this reason perhaps it is that pulmonary consumption is so fatal a complaint, for the parts being necessarily in constant motion, have not the opportunity afforded them of recovering from an attack of disease."

The second Section is, "*On the Causes and different Kinds of Strictures of the Rectum.*"

In the treatment of this, as in the former section, he refers back into the researches of the ancients; establishes his foundation upon this classic ground, and adroitly concludes in the following manner:

"First, it may be remarked, that we meet with no description of the disease, in any author that I can find, before the time of Wiseman; and the accuracy of the ancient writers of surgery, in their history of diseases, is universally allowed, whatever may be said of some of their modes of cure. It may therefore, I think, be fairly inferred, that either the complaint did not occur so frequently formerly, or that it was overlooked by physicians and surgeons, who suffered scarce any thing which related to diseases altogether to escape them. The case in Wiseman, indeed, was the consequence of the operation for the fistula in ano, as it was then performed; but, I believe, he was the first who has described any species of the disease, and he treated it in a way that was well worthy of the imitation of his successors, if they had sufficiently considered it: but I shall say more of this when I come to the treatment of the disease.

"Soon after Wiseman, as I have related, many authors have described the disease, and they all concur in considering it as inevitably fatal, until Mons. Petit\* gives a hint, that when it was a venereal symptom, it was curable, by the usual remedies for that disease; but that in other cases he had never seen it relieved.

"Dessault saw it so frequently in combination with other symptoms, decidedly venereal, that he did not hesitate at once to put his patients under a course of mercury, and with a success that fully warrants us in considering it as, very frequently, a symptom of the venereal disease. He did not, however, trust

\* *Loco Citato.*



solely to the effects of this remedy for a cure, for he saw that the particular functions and morbid alteration of the part, required a particular local treatment, independent of the medicine which was to cure the constitutional disease. He therefore, during the exhibition of mercury, every day introduced into the rectum tents imbued with some ointment, of a greater or less size, and for longer or shorter time, according to the circumstances of the case, and by this combined local and constitutional treatment generally cured his patient.

"Mons. Dessault does not seem, indeed, to consider the tent of so much importance in itself, but only as the vehicle of his medicaments. Be this as it will, however, he relieved his patients of a disease, which, till then, was generally considered as a fatal one.

"It should be remarked, that he ascribes the complaint to many other causes, besides the venereal disease, as to rheumatism, to gout, to cutaneous complaints; but notwithstanding this, he gave mercury, and mercury relieved them. Riche-rand\* also, in his *Nosographie Chirurgicale*, considers the stricture of the rectum as sometimes arising from the venereal disease.

"If the relief of a complaint by mercury (added to the use of the tent or bougie,) which is often accompanied by less equivocal symptoms of the venereal disease, and which has been considered as constantly fatal, be not thought sufficient to establish its syphilitic origin and nature, this opinion may, perhaps, derive some additional strength, when we reflect how often the neighbouring parts are attacked with complaints, of which no practitioner doubts the venereal origin.

"The condylomatous excrescences, which we see every day surrounding the anus; the large rhagades in these parts, though not so frequent a disease, are known to be venereal. And these, also, in addition to the constitutional remedy, require a local treatment adapted to their nature, as well as the stricture

\* Le retrecissement de l'extremite inferieure du rectum est quelquefois un vice de conformation; mais plus souvent l'effet de l'epaississement venereux de ses parois, de tous les symptoms de l'affection syphilitique il n'en est point de plus grave. *Nosographie Chirurgicale*, Paris, 1808, tom. iii. p. 418.

of the rectum. Even the fistula in ano is sometimes the consequence of the venereal infection,\* as has been remarked by Mr. Pott and Le Dran.

"After all, I do not by any means intend to assert, that it is always, or even most frequently, a symptom of the venereal disease. If it is met with in combination with other syphilitic complaints, there can be no doubt of the propriety of using mercury. And if it is relieved, in such cases, by mercury, added to the use of the bougie or tent, I think it is reasonable to employ, and to expect benefit from, the same means in other cases, where it may be the solitary symptom, and where we are disappointed in our methods of cure."

*The Treatment of the Stricture of the Rectum*, is the subject of the third Section.

"It has frequently been remarked (says Mr. Copeland) that he who knows a disease cannot be much at a loss how to treat it; and although this may not be true, perhaps, in its fullest extent, I believe no one will dispute, that the art of curing diseases advances, in some sort of proportion to the increase of our pathological knowledge." From this commencement in the treatment, Mr. C. proceeds, grounding all his observations and practical science on the comparative treatment of former authors, and their elucidations of the disease. After having advanced thus progressively, in stating what has been done by others, and weighing in his own judgment what the result upon the whole should be, he closes the subject with the following aphorisms, and thus concludes the third section.

"Before I quit this subject, it may not be useless shortly to recapitulate some of the most important circumstances of the disease and its treatment.

"First, That a stricture of the rectum is by no means so uncommon a disease as is usually imagined, and that it has been hitherto generally considered as necessarily fatal, because it has been discovered only in the last stages, or by dissection after death.†

\* Pott's Works, by Earle, vol. iii. p. 87. Le Dran, *Observ. de Chirurg.* tom. ii. obs. 84.

† See some observations on the diseased appearances of the rectum, by Dr. Baillie. *Morbid Anatomy*, page 111.



"Secondly, That many of those obstinate cases of constipated bowels, which are of long duration, arise from an organic obstruction to the passage of the fæces, and that this obstruction is most frequently so situated as to be within the reach of surgical aid.

"Thirdly, That it is requisite, in such cases, to examine the anus with the finger, or if the symptoms be strongly marked, and there be no obstruction within the reach of the finger, to examine it with a rectum bougie.

"Fourthly, That the use of internal medicines alone will be unavailing in such cases, and that nothing without the use of the bougie affords any hope of relief to the patient.

"Fifthly, That the disease is much less frequently of a cancerous nature, than from the description of authors it may seem to be.

"Sixthly, That it is often combined with symptoms of the venereal disease, and, in such cases, is more readily relieved by mercury, added to the use of the bougie, than by any other means.

"Seventhly, That if the disease is often found in combination with syphilitic symptoms, it is fair to infer that it may, in some cases, also, be the solitary symptom, and that if it resist the local treatment, and there be reason to suspect venereal mischief in the habit, it is right to try the effect of the mercury at the same time.

"Lastly, That whatever be the nature of the disease, provided it be not true cancer, it is necessary to continue the use of the bougie, at intervals, for a considerable time after the free passage of the fæces has been established, and to return to it whenever there be any symptoms of a recurrence of the complaint."

*The Fourth Section treats on the Hemorrhoidal Excrescence."*

Mr. Copeland very candidly states, that the subject of this section has been so very accurately described, and distinguished from other complaints, which it sometimes resembles, by sir James Earle and some others; and is a disease so well known to surgeons, that it becomes unnecessary to enter into a minute detail of its nature and appearances. But he adds, that he who has no other opportunity of informing himself on this subject,

than what is afforded him by the English writers, who have treated or touched on it, will be apt to conclude that the extirpation of such excrescences, either by ligature or excision, is always a safe and successful operation. Sir James Earle has powerfully urged, in his edition of the works of the immortal Percival Pott, the removal of them by ligature, and his cases were successful. And other surgeons have recommended excision, in cases of which the treatment has been also successful. But Mr. Copeland has found from practice, that the operations by ligature and excision in some cases have proved fatal. He has not relied upon his own bare assertion for the verification of these facts; but has applied his knowledge and his reading to the establishment of his own opinion by that of others. *Petit* has related cases, where the plausibility of success *a priori* was the most promising, and where by ligature, the patients died. From such instances, in similar cases, if there be any system adopted for removing these obnoxious and vexatious excrescences without a chance of incurring such unexpected and dangerous consequences, certainly Mr. Copeland has done well, and given us a lively instance of the value of that activity of mind, which was thus awake to so laudable a duty. If ligature and excision be sometimes from an unforeseen state of the constitution of a patient liable to produce fatal consequences, surely that method which does not expose a patient to the same danger, is and must be acknowledged to be an improvement. And here an opportunity is given to remark, that Mr. Copeland has displayed a great deal of that fire of genius which, when cultivated, must give us some idea of the character of what we expect from a true surgeon.

Mr. Copeland has a just apprehension of the hazards that are incurred both by excision and ligature in these cases; and points out a middle course which promises to avoid the danger of both; in which he is sanctioned by the authority of Hippocrates.

"Among the various modes of extirpating the tumors, which are suggested or recommended by Hippocrates, who has described the disease and its cure almost as well as any of his successors, that of taking them off (I suppose pinching them off, *αφαιρῆσαι* auferre,) with the fingers, deserves to be men-



tioned as a sort of medium between the ligature and the excision, comprehending, in cases where it is practicable, most of the advantages of each without the dangers of either. This rude operation of Hippocrates, (which he says may be done without telling the patient any thing of the matter,) or at least the essential principle of it, namely, of producing some kind of contusion at the time of removing the excrescence, is worthy of consideration. It is a well known fact, that the instinct of animals, directs them to bite off the umbilical chord, when they produce their young. The laceration which the parts suffer in this natural operation prevents all hemorrhage from the chord. The life of the young is thus preserved, until the circulation is accommodated to that alteration which is necessary after birth, when the fœtus becomes a perfect and respiring animal."

Among other remedies to which Mr. Copeland has called our attention, is the frequent injection of cold water up the rectum, with the addition of a few grains of sulphate of zinc, as employed by Schmucker. He likewise relies much upon the application of bougies in this complaint, as practised by Monteggia, an eminent Italian surgeon.

Though the works of Hippocrates are open to every surgeon, yet the merit of bringing into notice a fact from this father of the art, has been left to Mr. Copeland; a fact that will probably supersede the employment of excision or ligature.

We think, and it is but barely a presumption, if our author had pointed out that the common forceps, sharply dentated, would be the properest instrument to embrace the stem of the hemorrhoid, so that by the lacerating action of it, any hemorrhage would be prevented, that the direction would have been useful. And as to ligature, none being made, no consequence from it could follow. The forceps should be applied to bring forward the hemorrhoid, and on its division, all which is left within the gripe of the forceps will be sufficiently lacerated to stop all hemorrhage.

The fifth section treats of *Fistula in Ano*. The author very justly observes in a note, page 58:

"It is remarkable, that a disease of so frequent occurrence should have excited so little of the attention of the English sur-

gical writers. Fewer cases of the disease have been published, and fewer remarks on the infortunia and the anomalous circumstances attending it during the progress of the cure, than almost any other disease. Except the classical treatise of Mr. Pott, who has by no means exhausted the subject, there has hardly any thing appeared on this subject for the last century."

The point of this section goes chiefly to two circumstances, which render the operation of *Fistula in Ano* more complex and uncertain in its event. From the hemorrhage which sometimes follows; and from the cause of the disease sometimes being too remote to be removed by the operation. The hemorrhage has been known to be fatal. The stricture in the rectum with a morbid state of the inner membrane of the gut, is far from being less frequently fatal; and where this is the case, the fistula becomes only a symptom, or consequence of the original disease.

Our author says, " the degree of inconvenience, therefore, which the fistula produces, and the other circumstances of the more important disease, are to be taken into consideration before the operation be resolved on, in cases of this description.

And here also Mr. Copeland appeals to authorities which support his well intentioned improvement.

The latent sinuses, which are to be found in the complex cases of fistula in ano, have not been amply treated on. A dependence on the excellent direction of Mr. Pott has given a confidence to the operative surgeon which makes him blind to any farther inquiry; but it must be remembered that the operation can do no more than relieve the cause within its scope; and that the operation is not the less valuable from not doing more than what Mr. Pott even intended it should do.

The remaining section consists of well narrated Cases, illustrative of the author's observations on the several diseases of which he treats. In conclusion, we may venture to recommend this work to the profession, as containing the most correct and clear account of a class of diseases often obscure, and too frequently neglected.



## ORIGINAL PAPERS.

*On the use of the Vaccine Crust or Scab.*

I HAVE observed, with much concern, the difficulties attendant on vaccination for want of a continued supply of the vaccine virus, and therefore offer the following remarks on the most easy and effectual mode of communicating the infection at all seasons of the year.

*On the use of the crust or scab in Vaccination.*

When the wonderful discovery of vaccination was first made known by doctor Jenner, so desirous were physicians of proceeding with safety in this untrodden path, that they generally adopted the doctor's directions in using the limpid fluid taken from the vesicle, on the eighth day before the areola was formed. The danger of using the purulent matter secreted round the vesicle in the after stage of the process, appeared to be a sufficient reason for this cautious mode of procedure. But the difficulty of procuring vaccine virus in thus following doctor Jenner's prudent advice, operated as a real discouragement to the practice of vaccination. When the season for performing it arrived, the inquiries for the virus were anxiously made, and it was often necessary to procure it from a distance before the business could commence. We are now happily relieved from these embarrassments, for, in the year 1802, James Bryce of Edinburgh, surgeon to the vaccine institution of that place, published an account of his experience in using the scab in which the vesicle terminated—and this is justly considered as one of the greatest improvements on doctor Jenner's valuable discovery, as it furnishes us with a most certain mean of communicating the vaccine virus, and one which affords it at all times of the year.

The most perfect vesicles which go on to the state of crust or scab, without any deviation from the proper character; and which, when they fall off are somewhat transparent, smooth, of a mahogany color, and rather brittle than tenacious in their texture, are to be chosen to propagate the infection. Great care

is requisite to ascertain that it is really the vaccine crust or scab formed from the vesicle, which first falls off, as another scab of a different nature will often quickly form on the part. The scab should be wrapt up in a clean linen rag or white paper, and kept in a cool dry place. When used, the margin, which is of a lighter color, should be removed with a knife; and a portion of the remaining dark, hard, internal part is to be shaved off, reduced to powder on a piece of glass, and moistened with a small quantity of cold water, mixing it well together and then introducing it in the arm on the point of a lancet, leaving also a small portion of the scab on the scratched part. No more of the scab must be moistened at one time, than what is used, and no greater portion should be shaved off from the scab than what is wanted for the present occasion, as it appears to retain its strength better by continuing in the undetached state. It is believed that the livid vesicle, and especially the unopened one, is the most powerful.

By attending to the above directions I have lately vaccinated, from different scabs, twenty persons in succession, without a failure, on the first attempt. The frequent applications I have received, for vaccine virus, from many parts of the United States, occasion the present attempt to diffuse the knowledge of this easy and effectual mode of preserving it for use at all seasons of the year. The oldest scab I have ever used with success was one which I had possessed for eleven months. It is my present practice to vaccinate in every month of the year, and to use the scab only for this purpose.

It does not appear, by the European medical publications, that the vaccine scab is there much employed, but it is reasonable to hope that so certain and easy a mode of communicating the infection will soon come into general use.

SAMUEL P. GRIFFITTS.

Philadelphia, 3d mo. 9th, 1811.



*Account of a Fever which prevailed among the Indians on the  
Island of Nantucket, in 1763-4.*

BY JOSEPH PARRISH, M. D.

HAVING passed several days on the island of Nantucket in the summer of 1805, it afforded me an opportunity of making many inquiries respecting the epidemic fever which swept away almost two thirds of the Indians.

For the information on which the present narrative is founded, I am particularly indebted to Richard Mitchell an ancient and very respectable friend; who was one of the selectmen at the time of the calamity, also to the relatives of Zaccheus Macey (deceased), who obligingly permitted me to examine his manuscripts on the subject.

The disease was a very malignant typhus or ship fever, and appears to have been introduced in the following manner.

A snow arrived off the bar of Nantucket, having a number of passengers from Ireland. The first intelligence of any sick being in the vessel, was brought by several persons who came from an adjacent island, and reported that they saw two dead bodies floating on the sea. The selectmen were alarmed lest they should have the smallpox on board, and deputed several of the inhabitants who had passed through that disease to go and ascertain the fact; they returned with information that it was yellow fever.

The selectmen issued orders to the captain, to throw no more of the dead overboard, but to bury them on the shore, and sent off spades and shovels for that purpose; and further directed that no person from the vessel should enter the town.

Soon after this, information was lodged that several persons had left the vessel, and together with their baggage were at the house of Mary Quinn an Irish woman who resided in the town. As it was late in the evening when this intelligence was received, they were not ordered off until morning. When a messenger was despatched to Mary Quinn for that purpose.

The messenger found an Indian woman at the house engaged in washing the clothes of the passengers. She resided in one of their villages, in the family of the priest. In nine or at furthest eleven days, this woman was seized with a malig-

nant fever and died. The Indian priest and all his family took it and died, as well as most of the inhabitants of the village, and thence it gradually extended its ravages over the greater part of the island; as will appear, by an annexed statement of cases taken from the manuscript of Zaccheus Macey.

The sick had scarcely any medical assistance: the physicians declined attending among them. Richard Mitchell gave me the following account of symptoms.

Pain in the head, soon followed by a yellowness in the skin and eyes. A few hours before death the color would change to livid. The face and eyes, particularly of those who died with a short illness, were swelled in a shocking manner. Hemorrhages from the nose also occurred. Few or none recovered that were delirious. Some appeared to die in consequence of suffocation induced by tumefaction about the throat. In some cases the disease proved fatal in forty-eight hours. Some who passed the eighth or ninth day recovered. Swellings about the neck, which terminated in suppuration, were favourable signs: two instances of this kind were visited by Richard Mitchell: the ulcers healed kindly. Others who survived the first shock of the disease died eventually with chronic complaints.

Eight Indians who retired to a remote part of the island and avoided all intercourse with the sick, remained free from the disease. A number more, principally boys and girls resided in the town, among the white inhabitants, they were generally exempted from the calamity, but a few instances occurred of their visiting their sick relatives, taking the disease, and dying in the houses of their masters.

But what constitutes a very extraordinary part of this melancholy history, is the fact, that all the whites were exempted from the epidemic, except Mary Quinn, who it is said, had the disease but recovered.

It commenced in the eighth month (August) 1763, and continued until the succeeding second month (February); in this respect it differed essentially from what is commonly called the yellow fever.

The number of Indians belonging to and chiefly residing in



the island of Nantucket at the time of the calamity was	-	359
Of which had the disease	- - - - -	259
Died	- - - - -	223
Recovered	- - - - -	36
Lived among the sick and escaped the disease	- -	34
Retired to a remote part of the island and avoided all intercourse with the sick	- - -	8
Lived among the white inhabitants and escaped	- -	40
Absent at sea	- - - - -	18

From the same account it appears that in 1788 the number of Indians belonging to the island, was reduced to five males, and thirteen females. And the writer whose manuscripts extended as far back as 1722 records an observation, that is worthy of remembrance, viz. he never knew one of them to be deaf, but frequently blind.

*Experiments on the Mineral Waters of Bath, (Bristol, Penn.)*

By JAMES CUTBUSH.

Experiment

1. Litmus paper was not sensibly altered.
2. Turmeric paper remained unchanged.
3. Muriate of barytes did not produce a precipitate.
4. Acetite of lead gave a white precipitate, which, on standing, was increased.
5. Nitrate of silver produced a purple color, and, on standing, afforded a precipitate more or less colored.
6. Oxy-sulphate of silver produced a precipitate analogous to experiment 4.
7. Solution of soap remained unaltered.
8. Caustic potash gave a brown precipitate.
9. Caustic ammonia produced the same effect.
10. Carbonate of potash struck a brown color.
11. Nitrate of mercury afforded the same result as expt. 4.
12. Lime water did not produce a white precipitate, after standing some time.
13. Oxalate of potash remained unaltered.
14. Carbonate of ammonia and phosphate of soda, added one after the other, gave no precipitate.

## Experiment

15. Alcohol, added to an equal quantity of the water, produced no change.
16. Alcohol of galls produced a brown color, and, on standing, gave a black precipitate.
17. Prussiate of lime produced a blue color.
18. Triple Prussiate of potash afforded the same result.
19. Succinate of ammonia produced a brown precipitate.
20. Muriate of lime was not sensibly affected.
21. Solution of arsenic produced a light brown appearance.
22. Nitric acid did not produce any visible effect.
23. On exposing a portion of the water to heat, in an evaporating dish, an ochrous deposition was formed.
24. By separating the product of expt. 23, and adding alcohol of galls to the filtered liquor, no change of color ensued.
25. Sixteen ounces of the water was evaporated to dryness; the residue was collected and weighed; it amounted to 2.25 gr.; of which .75 was muriate of soda. The remainder (1.50 gr.) was oxyd of iron.
26. A piece of polished silver immersed in the water became tarnished. This effect, however, is not apparent until after some hours.

The temperature was ascertained to be the same as other spring water.

## RECAPITULATION.

The first experiment indicated the nonexistence of *uncombined* acid. Sulphuretted hydrogen, if in sufficient quantity, *and not in combination*, will also affect the color of litmus. The second experiment proved, that no alkali nor alkaline earth, if present, was in excess. That none of the sulphates, (salts formed with sulphuric acid) were held in solution, is inferred from expt. 3. Muriatic acid, however, was found to be present, from the production of muriate of lead. The fifth experiment indicated sulphuretted hydrogen, which existed in the state of combination, as will be hereafter noticed. It may be proper to remark, that the solutions of silver (the nitrate particularly) are precipitated by sulphuretted hydrogen, and by



hydro-sulphurets; but in this case, the precipitate, instead of being white, which is produced wholly by muriatic acid, is more or less of a purple or blackish color. The precipitate in this instance is soluble in dilute nitrous acid. Extractive matter has also this effect; but its presence was not detected.

The sixth experiment confirmed expt. 4th, in producing an insoluble muriate of silver.

The seventh experiment proved the nonexistence of earthy salt; if it had occurred, a turbidness would have ensued, owing to a mutual decomposition of the soap (saponule of soda) and earthy salt. If a metallic salt, such as a sulphate or muriate, had been present, a turbidness would have also taken place. Expts. 8, 9 and 10, showed the existence of metallic matter, which, by the use of other reagents was proved to be iron.

Experiment 11 confirmed expt. 4, by producing a submuriate of mercury. Expt. 12 proved, that no carbonic acid was present; if it had occurred, a white precipitate, soluble with effervescence in any of the acids, would have been formed.

Although none of the earths were detected by the reagents before mentioned, yet, in order to examine the water for lime and magnesia, the 13th and 14th experiments were instituted. These, however, were unsuccessful.

Alcohol produced no precipitate, which demonstrated, agreeably to the observations of Mr. Kirwan, and with the other reagents employed, that none of the sulphates were held in solution.

Alcohol of galls indicated the presence of iron. In consequence of the peculiar color it produced, being analogous to the phenomena which take place under similar circumstances, by which the solvent of the metal was inferred, the presence of sulphuretted hydrogen as the menstruum of the iron was proved to exist.

Experiment 17 and 18 confirmed expt. 16, by producing the Prussiate of iron; the 19th expt. also confirmed this conclusion, by affording a succinate of iron.

The 20th expt. proves, the nonexistence of uncombined alkali; the 21st the presence of sulphur, according to Westrumb;

and the 22d, that the substances present were soluble in this acid.

The 23d expt. also proves, that the solvent of the iron was a volatile, and not a fixed one; for, on the application of heat, the menstruum was carried off, and the iron deposited in the form of an oxyd.

Experiment 24 also confirms this conclusion.

The 25th expt. was instituted in order to ascertain the quantity of fixed ingredients which the water contained. Accordingly, on evaporating sixteen ounces, a residue, consisting of two and a quarter grains was left; after separating the saline matter, the remaining one and a half grains was the ferruginous oxyd.

Experiment 26th proved the presence of hepatic or sulphuretted hydrogen gas.

Considering these experiments and inferences as conclusive, as far as they have gone, it would follow, that this water is a chalybeate, (which has heretofore been proved,\*) that the iron is dissolved wholly, or in part, by sulphuretted hydrogen, and that it is accompanied by an alkaline muriate, the muriate of soda.

Bath, March 25th, 1811.

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### *Thoughts on Superfætation.*

By N. CHAPMAN, M. D.

By superfætation, we understand in the usual acceptation of the term, a conception, or conceptions succeeding to one which has already taken place, so that the uterus contains at the same time, two or more fætuses of very DIFFERENT AGES AND SIZES. Does superfætation, as thus defined, ever occur in the human species?† It was once very generally thought that it did occa-

\* See the experiments and observations of Dr. Rush, and Dr. de Normandie: the former in a treatise published in 1776, and the latter in the American Philosophical Transactions.

† Let me be understood. I mean where a perfectly natural organization of parts exists. Whether successive impregnations can take place in women who have a complete double set of genital organs, I will not take upon myself to decide. This is a distinct question. There are, however, some facts which



sionally happen, but the impression prevailed at a season, when in medicine, a sort of popular credulity had usurped the place of cautious investigation, and well tempered scepticism.

Discarded by the common consent of physiologists, for more than half a century, this notion has of late been revived, and has received the support of some few respectable writers. As having again become the subject of speculation, it may, perhaps, be not wholly unworthy of a brief examination.

It seems to me, that a belief in superfætation can hardly be entertained by any one who is conversant with the human economy, and particularly with the changes which the uterine system undergoes in consequence of pregnancy. We know that soon after conception, the os tinæ, as well as the internal apertures of the fallopian tubes are closed by a deposition of a thick tenacious mucus. But to accomplish still more perfectly, an end so important to the scheme of generation as the *occlusion* of the uterus, nature resorts to another provision.

Either by the sprouting forth of minute bloodvessels, or by the effusion of a species of lymph, or out of coagulated blood,\* a membrane of some firmness of texture is quickly formed. This membrane, which is called decidua, or caduca, from its being shed at the period of delivery, lines completely the uterus, and thus cooperates with the dense mucus already alluded to, in obliterating the three openings into its cavity.†

show that it is not wholly improbable. In his *Opuscula Pathologica*, Haller relates the case of a lady in whom was found, after death, a double uterus and vagina; and he is of opinion, that women thus formed, might be liable to a subsequent conception. Purcell gives an account in the *Philosophical Transactions*, of a similar case, and expresses the same conviction: and Lobstein, professor of anatomy at Strasburgh, tells us, in the *London Medical Journal*, that he actually delivered a woman of two children, one a month after the other, and was able to convince himself, that "this was owing to her having two uteri, to each of which there was a distinct vagina." Another instance of a *lusus naturæ* of this kind, is recorded by Mr. Pole, in the *Memoirs of the Medical Society of London*.

\* "The illustrious Haller supposed, that this was formed by naked vessels shooting out from the uterus. Dr. Hunter imagined, that the arteries of the uterus poured out coagulable lymph, which was afterwards changed into decidua. His brother, Mr. John Hunter, attributed its origin to coagulated blood, which formed a pulpy substance on the inner surface of the uterus." Burns's *Midwifery*.

† Burns on the Gravid Uterus.

Such, too, is the enlargement of the gravid uterus, and the change thereby produced in the relative position of its appendages, that a new series of impediments arise to the frustration of a second conception.

In this state of the organ, it is accurately ascertained, that the tubes lie parallel to its sides, and subsequently in the progress of gestation become bound in the same situation, instead of running in a transverse direction towards the ovaries, with their extremities loose and fluctuating.\*

Were an embryo, therefore, to be generated by any anomalous combination of circumstances, the tubes could not possibly embrace the fecundated vesicle, and the embryo, of course, must remain in the ovary, or fall into the abdomen, constituting an extra-uterine conception.

Let us, however, withdraw all the obstacles which have been enumerated to the passage of the embryo, and admit the practicability of its reaching the uterine cavity. What in this event would happen? Disorganization fatal to each fœtus must inevitably ensue.

It is to be recollected, that the uterus had prepared, in the first instance, whatever was required for the reception, the nourishment, and evolution of the fœtus. It had originally supplied it with a decidua, as a medium of attachment, and afterwards with a placenta for still more important purposes. To the second fœtus, the same offices are to be rendered. These it could not execute, without suspending the actions existing at the time, and taking on such as are necessary to the fabrication of an additional decidua and placenta.

That actions so incompatible cannot coexist, strikes me, as sufficiently obvious. Were the uterus, therefore, to attempt this new process, the result would be, the separation of the

\* "Soon after impregnation takes place, the cervix uteri becomes entirely shut by a thick viscid gluten: the internal cavity is also lined by the internal membrane of the ovum, which attaches itself to the whole internal surface of the fundus uteri. (1) The fallopian tubes also become flaccid, and are as gravity advances, supposed to be removed at such a distance that they cannot reach the ovaria to receive or convey another ovum into the uterus." *Edinburgh Practice of Midwifery.*

(1) The gravid uterus undoubtedly receives a membranous lining, but not exactly in the manner here described. *Vid. Burns on the Gravid Uterus.*



primary decidua and placenta, occasioning an abortion, accompanied with hemorrhagy, which would sweep out the whole of its contents.

It is probably, on this account, that menstruation uniformly ceases with the accession of pregnancy. I am aware, that this is a point not altogether conceded. The weight of authority is, however, decidedly against menstruation continuing during gestation. By all the very recent writers it is denied. Those who hold, or I might rather say, did hold the contrary opinion, have mistaken a hemorrhagy from the vagina, which sometimes recurs with considerable periodical regularity, for the catamenial flux. Several cases of this kind have come under my own observation, where I had an opportunity of inspecting the discharge accurately. In every instance, I found it pure coagulable blood, having neither the color, nor odor, nor any other of the peculiar properties of the genuine menstrual fluid.

By again adverting to the condition of the pregnant uterus, we shall see that a suppression of the catamenia is exactly what ought to be expected. The deciduous membrane is framed while the process of conception is proceeding in the ovary. The vessels which had secreted the catamenia are now engaged in a new operation. They form the membrane, and then support it. While thus employed, their secretory function is suspended. They cannot, at the same period, perform actions so incongruous and inconsistent. The one must yield to the other. This is very strikingly illustrated by the fact which has not been sufficiently attended to, that in a large proportion of the cases of obstinate amenorrhea, the membrana decidua exists, and that the first symptom of the return of the discharge is the coming away of the membrane. Of the identity of the two membranes, there can be no doubt. It has been determined by very competent judges.\*

By one less averse than myself to speculative reasonings in matters of science, a variety of considerations of this nature, might be pressed against the hypothesis which I am combating. It could, I think, in particular, be urged with great plausibility, that changed as is the whole uterine system by ges-

\* Baillie. Burns, &c.

tation, not only in the mechanical distribution, but also in the structure and functions of its parts, it cannot possibly assume that peculiar condition which seems indispensable to conception. Of all the operations of the animal economy, that of conception, undoubtedly requires the most harmoniously concerted action in the several organs by which it is commenced, carried on, and consummated. Derangements in any one portion of this complex apparatus are confessedly productive of sterility. So essential, indeed, to the generative process in the human species, is a perfect integrity in the functions of the uterine system, that by the suppression or even vitiation of the catamenia, the aptitude to conception is lost or diminished. But enough of these speculations. I am content to rest the defence of the question on the facts which I have stated. If they be correct, it results that superfætation cannot take place in the human species.

As, however, it might not be candid to close the present discussion without noticing the arguments which have been adduced to support the opposite views of the subject, they shall now, as far as I am acquainted with them, be detailed.

The believers in superfætation have relied, with no inconsiderable confidence, on the analogy of those animals, which admit during the season of procreation various males in succession, and produce offspring marked by the respective peculiarities of the male parents. Thus, it is affirmed, and with truth, that a bitch while in heat, will be connected with several dogs of very opposite descriptions; as the spaniel, the cur, the mastiff, the hound, the terrier, &c. and bring forth a litter of pups resembling more or less each breed.

This, though an imposing argument on the first aspect, is wholly fallacious. There is here not a case of *superfætation*, as is imagined, but of *contemporaneous conception*. Experiments carefully made, prove incontestably, that the vesicles of the ovary in multiparous animals are ripened, and the elementary matter of each ovum transmitted simultaneously to the cavity of the uterus.

We have too, in confirmation of these experiments, the acknowledged fact, that at the time of birth, the whole brood is alike matured, having in an equal degree, the indications of fœtal completeness.



But there are other circumstances of which we must not lose sight in this investigation. The uterus of the bitch, and I presume, of all the animals which multiply in a similar mode, differs very materially from the same viscus in the human species. That of the former is divided into a number of cells or compartments, each of which may be deemed to a certain extent, an independent organ. These animals have, moreover, no deciduous membrane, and hence the ova do not become attached to the uterus for some length of time after arriving in its cavity.\*

Even, therefore, were we to allow, in contradiction to positive experiments, that successive conceptions are incident to the generative process in one class of animals, the concession would be intitled to no sort of weight in deciding the question as regards the human species.

But the advocates of superfætation have not left the vindication of the hypothesis solely to this loose, remote, and inapplicable analogy. They strenuously appeal to the cases, where two or more children of different sizes, and *seemingly of different ages*, have been born nearly at the same time, or after a longer interval. That such instances have been observed, I am not disposed to deny. They are, however, exceedingly rare, and have seldom been accurately related, or well authenticated. Few, indeed, have of late been recorded. The writers of the "olden time" abound with them; but their reports are so habitually mixed and disfigured by the extravagance of intentional falsehood, or the delusions of credulity, that it is difficult to distinguish the truth from the fiction, or to pronounce between the reality and the deception.

In a majority of the instances of reputed superfætation, really deserving of credit, the lesser child is represented as *dead and decayed*. The explanation of such cases is too obvious to require being particularly indicated. It will at once occur to every one, that the fœtus died prematurely, and was retained in the uterus. We have, however, been sometimes told, that the diminutive child was born alive, or if dead, had all the

\* "The uterus of the bitch is divided into different cells, and their ova do not attach themselves to the uterus so early as in the human subjects." *Edinburgh Practice of Midwifery*, &c.

appearances of recent dissolution. Where the child was alive, I should impute the smallness of its size either to original defect of constitutional vigor, or to retarded growth and development; and if dead, to disease, or to one of those numerous accidents to which it is perpetually exposed. It has long been known, that the fœtus in utero is subject to many of the most violent diseases. They have come into the world covered with smallpox, deformed by syphilis, and even with calculi in the urinary bladder. But it may be asked why, if the smaller fœtus were thus early deprived of vitality it did not exhibit the signs of putridity? To this interrogatory the answer is prompt. When the membranes of the ovum are entire, so as to exclude air, the process of putrefaction in the fœtus is resisted during gestation.\*

My explanation of each description of the cases to which I have alluded, may, after what has been said, be comprised in a few words. Twins were conceived. At an early stage of pregnancy one of the fœtuses perished, or its evolution was interrupted. As twins are inclosed in a separate involucre, and are nourished by different placentæ or distinct portions of the same organ, as often happens, the one fœtus may not at all be affected by circumstances which might prove mischievous or fatal to the other.

But there is still another order of cases which is occasionally summoned to sustain this hypothesis. It is asserted, that women have brought forth two full grown children at the same birth, the one *white* and the other *black*, the product of a connexion in immediate succession, with a black and a white man. I have met in my researches but with three recorded instances of this sort, and they come in a very "questionable shape." No one of the historians of these marvellous stories pretends to have witnessed such a case. They are given merely on the authority of hearsay, or popular report. As related, the cases indeed carry with them internal evidence of condemnation. There could not, under the alleged circumstances of the connexion, be a *white* and a *black* child, but one must have been a *mulatto*, according to the invariable result of a fruitful intercourse between the negro and white!

\* Smellie's Midwifery. Cases, in corroboration of this fact, are indeed familiar to every obstetrical practitioner.



But if substantiated, what would these cases avail in the present controversy? They are not instances of superfotation. To bring them within the proper meaning of the term, it must be shown, that the children were of "very different ages and sizes." But as the reverse is stated, they can only, even if true, be classed with other examples of *contemporaneous conception*.

It appears to me, on the whole, that this hypothesis has not the slightest claims to our attention. It comports neither with reason, fact, nor analogy; and is repugnant to the established laws of generation, and the uniform course of nature.

Philadelphia, March 25th, 1811.

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#### ORIGINAL REVIEW.

"*An Account of the remarkable Effects of the Eau Medicinale d'Husson, in the Gout. By Edwin Godden Jones, M. D. Member of the Royal College of Physicians of London, and Physician extraordinary to his Royal Highness the Duke of York.*" Second Edition. London.—1810. pp. 101. 12mo.

"THE Eau Medicinale was discovered about forty years ago, by M. Husson, a military officer in the service of the king of France. He discovered a plant, whose virtues were before unknown, from which he prepared his medicine in its present form."

It has been analyzed by eminent chemists in France and England, and found to be a vinous tincture of some unknown vegetable, without any portion of a mineral substance. It is sold in Paris, where only it is prepared, and in London in small bottles which will hold little more than two drachms by measure—it is of the color and appearance of ale, but becomes brown and turbid on agitation. Its taste is nauseous and bitter, but not intensely so, nor does it leave a durable impression in the mouth, as many bitters do. Its smell partakes of that of Spanish wine, the menstruum employed, and that of the plant, its ingredient, which is the basis of the composition. It is strong and very peculiar and characteristic.

The whole bottle, mixed with more than an equal quantity of water is a full dose—to be taken on an empty stomach, at

any time of day, but night is the most convenient. Its operation may be advantageously promoted by a free use of any aromatic tea.

It happens for the most part, that in four or five hours after taking the remedy, the patient begins to experience a diminution of pain, however severe the paroxysm may be. He generally falls into a quiet sleep, and awakes in the morning, nearly or quite free from suffering; and often begins already to enjoy some returning use of the affected limb. About this time he commonly feels a considerable nausea, sometimes accompanied by vomiting, and this is followed by some bilious stools. In the mean time the paroxysm goes on diminishing; and on the third, or even on the second day, little more of it remains than a swelling or stiffness of the parts, which soon go off leaving the patient in his usual state of health.

With the diminution of pain, there is an abatement of fever and irritation, and of the action of the heart and arteries. The pulse is often reduced twenty strokes in a minute, and often more—a moderate diaphoresis takes place frequently.—It also often acts as a powerful diuretic, and for many days.

This remedy is most useful when early applied. It may, however, be given at any period. It sometimes merely removes the present paroxysm—and to prevent returns of the disease it may be occasionally used in smaller doses.

The Eau Medicinale is so far a cure for the gout that by it we may almost certainly remove the paroxysms, as often and as soon as they occur; many persons by this remedy have been able to cut off the attacks of gout in their very beginning, and it is very probable, that, by employing it in doses adapted to circumstances, and by a proper regimen, even the desirable object of preventing the return of the fits may be in a great measure accomplished.

It has been dreaded that the Eau Medicinale might eventually produce the same fatal consequences, that have been attributed to the famous Portland Powder, or to other applications, which are supposed to drive it from the extremities to some vital part, but it acts not by local effects, but on the system generally; and no instance has occurred of dangerous consequences from its use.



Let us hope that the composition of this remedy will not always be concealed; and that if it should continue to deserve the high reputation it has lately acquired, the time is not far distant, when the French government will, by rewarding the proprietor, obtain possession of the secret, and publish it for the general benefit of mankind.

A number of cases are given to show the efficacy of the remedy. It was first used by John Crawford, Esq. of Auchinames, to whom the introduction of it into England in 1804, is chiefly owing. This gentleman first heard of it at Montpellier in 1802, from Dr. Chretien, a physician of that place. A number of publications on the remedy, have appeared in France, at different intervals. Amongst the names of persons cured in London by its use, those of Sir Joseph Banks, and Major Rennell, with a number of the nobility appear.

We do not undertake to give an opinion on the value of this publication—conceiving that the most useful employment of a reviewer is to let the author tell his own tale—and leave the reader to judge of its merits. The subject is surely an important one; and when we consider how much time and talent are requisite to establish the virtues of a new medicine, it is to be hoped that the present one will not be hastily overlooked, because its composition is hitherto unknown—and more especially as we have very lately learned, that it continues to be well thought of by the most respectable London practitioners.

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*Biographical Notice of Dr. Osborne.*

In the month of August 1808, died at his country house near Dover, William Osborne, M. D. aged 76. Dr. Osborne was born in the county of Rutland, where, after obtaining a good classical education, he received the early part of his medical acquirements under the guidance of Dr. John Fordyce, of Uppingham. After passing some years with that able instructor, he came to London, and attended the lectures of the late Dr. William Hunter, and the practice of St. George's Hospital. His industry in the pursuit of knowledge was much

assisted by a memory uncommonly retentive, and a happy art of arranging all that he read and saw, so as to be able always to produce it with great readiness when occasion required. This faculty, aided by a well regulated judgment, probably contributed, in a great degree, to that celebrity which he afterwards attained in his profession. Having finished his studies in London, he went to Paris, where he attended the practice of the Hotel Dieu, and the teachers at that time in highest estimation, particularly the celebrated accoucheur, Levret. This gave a bias to his mind, which, fortunately for the interests of mankind, led him into that department of medicine, in which he maintained for many years, as long as his health permitted, the highest character. On his return from France, he took a professional situation in the guards, and went with the regiment to Germany, in the war at the end of the last and beginning of the present reign. It is remarkable that many of the young men at that time engaged in the army, in the service of their country, rose afterwards to the highest honours, and most distinguished situations in the metropolis. At the conclusion of the war Mr. Osborne settled in London, as a surgeon; but his studies at Paris gave a bias to his mind, and led him to devote his attention to the improvement of midwifery, and the investigation of the diseases incident to women and children. A similarity of pursuits and opinions produced a connexion between him and Dr. Denman, which led to their association in a plan of opening a school in London, for teaching the principles of Midwifery on a liberal and extended scale, and rescuing the practice from ignorance and temerity. The celebrity which their lectures acquired, is a strong proof of their fitness for so laudable an undertaking; and the number of pupils from every quarter of the world, showed how well their well-earned fame was appreciated. To the combined influence of public teaching, and to the scientific publications of Dr. William Hunter, Dr. Osborne and Dr. Denman, we are indebted for many improvements in the practice of midwifery, and a better knowledge as well as a more simple and effectual treatment of the diseases of women and children. Dr. Osborne soon rose to great eminence in the practice of his profession, for which he was not more qualified by the powers of his mind, than by a



peculiar urbanity of manners and deportment, and a disposition naturally tender and humane. The extent of his practice and the time he devoted to study, did not leave him much leisure for writing. He has, however, left behind him a work of great practical utility, on natural and laborious parturition, which will rank him among our best medical writers, whether the elegance and perspicuity of the style, or the manner of treating the subject be considered. The fatigue necessarily attendant on a laborious profession slowly impaired his constitution, and brought on complaints of the stomach, and palpitation of the heart, which induced him about the year 1800 to retire to his estate at Old Park, near Dover, where he acted as a magistrate for the county of Kent, and was always ready to afford medical assistance to the poor in his own neighbourhood. Here he lived in learned leisure, in the bosom of his family. About two months before his death, the symptoms of great debility in the stomach rapidly increased: the disease about his heart was very much aggravated, and he was well aware it would terminate fatally. He looked forward to his death with great composure and resignation, and with the humble confidence of a christian. He retained the powers of his mind and the vivacity of his disposition to the last moments of his life, and died almost without a struggle. The whole tenor of his life was useful to society; and his death was such an one as good men might envy.—[*Edinburgh Med. and Surg. Jour.* vol. v. p. 28.]

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#### MEDICAL INTELLIGENCE.

##### *National Vaccine Establishment.*

From the Annual Medical Register, for 1808.

It was understood, after the very decided tone, which the College assumed in their reports to the House of Commons, relative to the efficacy of the cow pock, that the Royal Jennerian Society, which was instituted and had flourished under the patronage of their Majesties, and of the whole Royal Family, would immediately have received the support of government. But government, it should seem, can only act through corporate organs; the work of vaccination has been

put into the hands of the President and Censors, for the time being, of the College of Physicians, and of the Master, and two other officers of the College of Surgeons, who constitute a Vaccine Board. The Jennerian Society necessarily falls to the ground; for the contributions of individuals are naturally withdrawn, when it is known that the public purse is open for the prosecution of a similar purpose. The sum of 3000 pounds, for the year, was voted by Parliament. An expensive *ready-furnished* house has been hired in Leicester Square, for the residence of the Register (who is also Register of the College of Physicians), and for the meetings of the Board; but no vaccination is there permitted: apartments in a house in Lisle Street having been likewise hired for the purpose of a central inoculating station, and for distributing the vaccine virus gratuitously. Dr. Jenner was not made one of the Board; but had the purely nominal appointment of *Director* assigned to him, which, however, he has declined.—Mr. Moore was appointed *sub-director*, and six inoculators were nominated to perform vaccination gratuitously in different districts, in London; to whom, we believe, a seventh has been added, for the Borough. Among these are Mr. Carpue, Mr. Charles Aikin, Mr. Lane, of Guilford Street, Mr. Vincent, Assistant Surgeon to St. Bartholomew's hospital, Mr. Sawrey, of Chancery Lane, and Mr. Halls, of Westminster. Mr. Murray, the active Secretary of the late Jennerian Society, has been appointed Secretary to the Board.

Although this establishment has, in our opinion, been constituted in a very objectionable manner, we are happy to hear, that some active measures have already been adopted for making its existence and objects known; we trust that this new energy will continue unabated; and perhaps the annual change in the Board, with the changing Censors, &c. of the Colleges, may be the best pledge for its continuance. Letters addressed to the Register, Dr. Hervey, 21, Leicester Square, will obtain for medical practitioners, in the country, a supply of vaccine virus, which will be sent to them, free of postage.



*Case of Calculus removed by Dilatation from the Female Bladder.*

From the London Medical Review.

A woman named Keen, who had not been able to retain her urine since her last delivery, which was a year ago, was lately admitted into Guy's Hospital for the stone.

On the 21st of last June, a piece of sponge was, by order of Mr. Astley Cooper, passed into the meatus urinarius, and on the 22d the sponge was withdrawn and a pair of middle sized stone forceps, were easily passed into the bladder, and a stone of one inch and a half long by one inch wide was extracted.

On the 27th she was discharged from the hospital, free from every symptom of stone, but the incontinence of the urine continues as before the operation.

Mr. Cooper acknowledged to the pupils that he was induced to make this trial by Mr. Thomas's interesting paper on the dilatability of the meatus urinarius, and rectum, published in the Medico-Chirurgical Transactions.

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*Prize Medals,*

OFFERED BY THE HUMANE SOCIETY OF PHILADELPHIA.

The success which has hitherto attended the efforts made to restore suspended animation has been highly encouraging, but it is well known that these endeavours are often unavailing. It remains then not only to strive to discover some more efficient means for the attainment of this desirable purpose, but also to ascertain the time and circumstances in which such attempts may be made. The managers of the Society, although disappointed in not receiving a successful communication, yet do not feel discouraged. They are aware of the difficulty of the subject, but fully convinced of its importance. Under these impressions, they are induced to offer,

For the best Dissertation on the means of restoring to life persons apparently dead by Drowning, and more effectual than





*Vaccine Society.*

A society for promoting Vaccination was instituted in this city two years since. Collectors are employed, who call on the poor throughout the city and suburbs and report such as require vaccination to the physicians of the society, who vaccinate them at their respective habitations. Between two and three thousand persons have already been vaccinated. A regular account of the cases is preserved.

*Statement of Deaths, with the diseases and ages, in the City and Liberties of Philadelphia, from the 1st of January 1810, to the 1st of January 1811.*

DISEASES.	Under 1 year	From 1 to 2	From 2 to 5	From 5 to 10	From 10 to 20	From 20 to 30	From 30 to 40	From 40 to 50	From 50 to 60	From 60 to 70	From 70 to 80	From 80 to 90	From 90 to 100	From 100 to 110	Total
Abortion,	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Asthma,	1	0	1	0	0	1	1	1	1	2	2	1	1	0	11
Abscess,	0	0	0	0	1	2	2	0	2	0	0	0	0	0	7
Aneurism,	0	0	0	0	1	0	1	0	0	0	0	0	0	0	2
Apoplexy,	2	2	1	0	2	2	5	7	5	3	1	0	1	0	31
Atrophy,	3	2	1	0	1	1	2	1	2	1	0	0	0	0	15
Burns,	0	2	4	4	0	0	0	0	0	1	0	0	0	0	13
Cancer,	1	0	0	0	1	0	2	1	1	1	1	0	0	0	8
Caries of the Spine,	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1
Casualties,	0	2	0	1	1	5	3	3	1	0	0	0	0	0	16
Catarra,	2	0	1	0	0	2	0	0	0	2	1	0	0	0	8
Child Bed,	0	0	0	1	0	0	0	0	0	0	0	0	0	0	1
Cholera Morbus,	129	49	12	11	0	2	0	0	1	1	0	1	0	0	206
Colic,	2	0	0	0	1	3	2	0	0	1	1	0	0	0	10
Consumption of the Lungs,	8	8	6	120	80	82	53	1624	9	2	0	0	0	0	306
Contusion,	1	1	0	0	0	1	0	0	0	0	0	0	0	0	3
Convulsions,	141	13	9	4	0	3	7	4	1	0	1	0	0	0	183
Chicken Pox,	0	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Decay,	7	6	3	2	3	5	23	6	16	7	10	5	0	0	95
Diabetes,	0	0	0	0	0	1	0	0	0	0	0	0	0	0	1
Diarrhea,	4	4	3	1	0	2	0	2	0	1	0	1	0	0	18
Dislocations,	0	0	0	1	0	0	0	0	0	0	0	0	0	0	1
Dropsy,	0	1	2	0	2	3	9	8	4	0	4	3	0	0	42
Dropsy of the Breast,	3	4	2	1	4	8	6	8	2	0	0	0	0	0	43
Dropsy in the Brain,	11	9	11	8	2	1	1	0	1	0	0	0	0	0	44
Drowned,	1	0	0	2	7	4	0	2	2	2	0	0	0	0	26
Dysentery,	5	6	3	2	1	2	1	2	2	0	3	0	0	0	27
Drunkenness,	0	0	0	0	1	3	4	3	0	1	0	0	0	0	12
Diseases in hip joint,	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Debility,	15	3	0	0	1	1	6	3	4	6	4	1	0	0	44
Epilepsy,	0	0	0	0	1	1	2	0	1	0	0	0	0	0	5
Eruptions,	0	0	0	2	0	0	0	0	0	0	0	0	0	0	2
Erysipelas,	0	0	0	0	0	0	1	1	1	0	0	0	0	0	3
Fever,	6	2	1	0	1	5	13	0	1	1	0	1	0	0	31
Fever Intermittent,	0	1	0	0	0	0	1	0	1	0	0	0	0	0	3
Fever Remittent,	1	1	3	0	3	1	3	2	0	0	0	0	0	0	17
Fever Bilious,	0	1	0	2	2	6	1	1	2	0	0	0	0	0	15
Fever Nervous,	1	0	0	0	2	2	1	0	0	1	0	0	0	0	7
Fever Malignant,	0	0	0	0	0	1	1	1	0	0	0	0	0	0	3
Fever Typhus,	0	0	1	3	1	3	15	5	4	5	0	1	0	0	39
Fever Puerperal,	0	0	0	0	0	8	4	0	0	0	0	0	0	0	12
Fever Hætic,	0	0	0	0	0	0	1	6	0	0	0	0	0	0	1
Fever Scarlat,	0	0	0	1	0	0	0	0	0	0	0	0	0	0	2
Fever Inflammatory,	1	0	2	0	0	0	0	2	0	0	0	0	0	0	5
Mortification and Gangrene,	3	0	2	0	0	3	2	1	5	1	1	0	0	0	18
Gout,	0	0	0	0	0	0	0	0	0	1	1	0	0	0	1
Gravel,	9	0	0	0	0	0	0	1	0	0	1	1	0	0	3
Whooping Cough,	15	11	6	0	0	0	0	0	0	0	0	0	0	0	32
Hives,	32	7	6	1	9	0	0	1	0	0	1	1	0	0	49
Hernia,	0	0	0	0	0	0	0	0	0	1	0	0	0	0	1
Hæmorrhage,	0	0	0	0	0	0	2	1	3	0	1	0	0	0	7
Inflammation of the Brain,	2	1	2	1	1	2	5	1	0	1	0	0	0	0	16
Inflammation of the Lungs,	0	1	0	0	1	0	2	2	4	1	0	1	0	0	12
Inflammation of the Stomach,	1	1	3	0	2	2	6	3	2	3	2	0	0	0	25
Inflammation of the Bowels,	7	3	2	1	1	5	7	6	2	3	1	0	0	0	38
Inflammation of the Liver,	1	0	2	2	0	2	8	1	4	0	0	0	0	0	20
Inflammation of the Bladder,	0	0	0	0	0	0	0	0	1	0	0	0	0	0	1
Insanity,	0	1	0	0	0	7	7	8	5	2	0	0	0	0	29
Jaundice,	0	0	0	0	0	1	2	1	0	0	0	0	0	0	4
Locked Jaw,	1	0	0	0	0	2	0	0	0	0	0	0	0	0	3
Lethargy,	0	0	0	0	0	0	0	0	0	0	1	0	0	0	1
Measles,	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Old Age,	0	0	0	0	0	0	0	0	0	1	9	25	7	2	44
Palsy,	0	0	0	0	1	0	0	0	4	1	4	0	0	0	10
Pleurisy,	9	5	4	3	2	10	9	15	4	7	3	1	0	0	73
Rheumatism,	0	0	0	0	1	1	1	1	1	1	0	0	0	0	6
Rickets,	0	1	0	0	0	0	0	1	0	0	0	0	0	0	2
Scrofula,	0	3	2	0	2	1	2	0	0	0	0	0	0	0	10
Sore Throat,	4	4	1	2	0	3	0	0	0	1	1	0	0	0	17
SMALLPOX--NATURAL,	10	5	3	2	0	11	2	0	0	0	0	0	0	0	33
SmallPox, Inoculated,	1	0	0	0	0	6	0	0	0	0	0	0	0	0	1
Still Born,	139	0	0	0	0	0	0	0	0	0	0	0	0	0	139
Suicide,	0	0	0	0	0	2	0	1	2	1	0	0	0	0	6
Sudden,	3	0	0	0	0	1	8	1	3	5	1	0	0	0	44
Syphilis,	0	1	0	0	0	6	1	2	1	0	0	0	0	0	11
Tabs Nephritica,	0	0	0	0	0	1	0	0	0	0	0	0	0	0	1
Teething,	4	3	0	0	0	0	0	0	0	0	0	0	0	0	7
Thrush,	1	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Tumors,	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Ulcers,	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Worms,	0	0	0	0	0	0	0	1	0	0	0	0	0	0	1
Unknown,	3	2	2	2	0	0	0	0	0	0	0	0	0	0	15
	4	1	3	1	0	6	6	4	0	5	1	0	0	0	31



*Deaths in each Month, of the above Period.*

	Adults.	Children.	Totals.
January, - - - -	82	67	149
February, - - - -	82	70	152
March, - - - -	87	40	127
April, - - - -	96	56	152
May, - - - -	99	65	164
June, - - - -	68	79	147
July, - - - -	63	129	192
August, - - - -	91	178	269
September, - - - -	91	82	173
October, - - - -	104	89	193
November, - - - -	86	66	152
December, - - - -	87	79	166
Total - - - -	1036	1000	2036

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No. IV.

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SELECTED PAPERS.

*Researches on the Oxymuriatic Acid, its Nature and Combinations; and on the Elements of the Muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution.\**

By H. DAVY, Esq. Sec. R. S. Prof. Chem. R. I. F. R. S. E.

From the London Philosophical Magazine, Vol. 36.

**THE** illustrious discoverer of the oxymuriatic acid considered it as muriatic acid freed from hydrogen,\* and the common muriatic acid as a compound of hydrogen and oxymuriatic acid; and on this theory he denominated oxymuriatic acid dephlogisticated muriatic acid.

M. Berthollet,† a few years after the discovery of Scheele, made a number of important and curious experiments on this body; from which he concluded, that it was composed of muriatic acid gas and oxygen; and this idea for nearly twenty years has been almost universally adopted.

Dr. Henry, in an elaborate series of experiments, made with the view of decomposing muriatic acid gas, ascertained

\* Mem. Acad. Stockholm for 1774, p. 94.

† Journal de Physique, 1785, p. 325.



that hydrogen was produced from it by electricity; and he attributed the phenomenon to water contained in the gas.\*

In the Bakerian lecture for 1808, I have given an account of the action of potassium upon muriatic acid gas, by which more than one-third of its volume of hydrogen is produced; and I have stated, that muriatic acid can in no instance be procured from oxymuriatic acid, or from dry muriates, unless water or its elements be present.

In the second volume of the *Mémoires d'Arcueil*, MM. Gay Lussac and Thenard have detailed an extensive series of facts upon muriatic acid and oxymuriatic acid. Some of their experiments are similar to those I have detailed in the paper just referred to; others are peculiarly their own, and of a very curious kind: their general conclusion is, that muriatic acid gas contains about one quarter of its weight of water; and that oxymuriatic acid is not decomposable by any substances but hydrogen, or such as can form triple combinations with it.

One of the most singular facts that I have observed on this subject, and which I have before referred to, is, that charcoal, even when ignited to whiteness in oxymuriatic or muriatic acid gases, by the Voltaic battery, effects no change in them; if it has been previously freed from hydrogen and moisture by intense ignition in vacuo.

This experiment, which I have several times repeated, led me to doubt of the existence of oxygen in that substance, which has been supposed to contain it above all others in a loose and active state; and to make a more rigorous investigation than had been hitherto attempted for its detection.

If oxymuriatic acid gas be introduced into a vessel exhausted of air, containing tin; and the tin be gently heated, and the gas in sufficient quantity, the tin and the gas disappear, and a limpid fluid, precisely the same as Libavius's liquor, is formed: it occurred to me, that if this substance is a combination of muriatic acid and oxyde of tin, oxyde of tin ought to be separated from it by means of ammonia. I admitted ammoniacal gas over mercury to a small quantity of the liquor

\* Philosophical Transactions for 1800, p. 191.

of Libavius; it was absorbed with great heat, and no gas was generated; a solid result was obtained, which was of a dull white color; some of it was heated, to ascertain if it contained oxyde of tin; but the whole volatilized, producing dense pungent fumes.

Another experiment of the same kind, made with great care, and in which the ammonia was used in great excess, proved that the liquor of Libavius cannot be decomposed by ammonia; but that it forms a new combination with this substance.

I have described, on a former occasion, the nature of the operation of phosphorus on oxymuriatic acid; and I have stated that two compounds, one fluid and the other solid, are formed in the process of combustion, of which the first, on the generally received theory of the nature of oxymuriatic acid, must be considered as a compound of muriatic acid and phosphorous acid. It occurred to me, that if the acids of phosphorus really existed in these combinations, it would not be difficult to obtain them, and thus to gain proofs of the existence of oxygen in oxymuriatic acid.

I made a considerable quantity of the solid compound of oxymuriatic acid and phosphorus by combustion, and saturated it with ammonia, by heating it in a proper receiver filled with ammoniacal gas, on which it acted with great energy, producing much heat; and they formed a white opaque powder. Supposing that this substance was composed of the dry muriates and phosphates of ammonia; as muriate of ammonia is very volatile, and as ammonia is driven off from phosphoric acid, by a heat below redness, I conceived that, by igniting the product obtained, I should procure phosphoric acid; I therefore introduced some of the powder into a tube of green glass, and heated it to redness, out of the contact of air, by a spirit lamp; but found, to my great surprise, that it was not at all volatile nor decomposable at this degree of heat, and that it gave off no gaseous matter.

The circumstance, that a substance composed principally of oxymuriatic acid and ammonia should resist decomposition or change at so high a temperature, induced me to pay particular attention to the properties of this new body.



It had no taste nor smell; it did not seem to be soluble, nor did it undergo any perceptible change when digested in boiling water: it did not appear to be acted upon by sulphuric, muriatic, or nitric acids, nor by a strong lixivium of potash. The only processes by which it seemed susceptible of decomposition were by combustion, or the action of ignited hydrat of potash. When brought into the flame of a spirit lamp and made red-hot, it gave feeble indications of inflammation, and tinged the flame of a yellow color, and left a fixed acid having the properties of phosphoric acid. When acted on by red-hot hydrat of potash, it emitted a smell of ammonia, burnt where it was in contact with air, and appeared to dissolve in the alkali. The potash which had been so acted upon gave muriatic acid, by the addition of sulphuric acid.

I heated some of the powder to whiteness, in a tube of platina; but it did not appear to alter; and after ignition gave ammonia by the action of fused hydrat of potash.

I caused ammonia, made as dry as possible, to act on the phosphuretted liquor of MM. Gay Lussac and Thenard; and on the sulphuretted muriatic liquor of Dr. Thomson; but no decomposition took place; nor was any muriate of ammonia formed when proper precautions were taken to exclude moisture. The results were new combinations; that from the phosphuretted liquor was a white solid, from which a part of the phosphorus was separated by heat; but which seemed no further decomposable, even by ignition. That from the sulphuretted liquor was likewise solid, and had various shades of color, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia; but as these compounds did not present the same uniform and interesting properties as that from the phosphoric sublimate, I did not examine them minutely: I contented myself by ascertaining that no substance known to contain oxygen could be procured from oxymuriatic acid, in this mode of operation.

It has been said, and taken for granted by many chemists, that when oxymuriatic acid and ammonia act upon each other, water is formed; I have several times made the experiment, and I am convinced that this is not the case. When about 15

or 16 parts of oxymuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gases, and from five to six parts of nitrogen are produced; and the result is dry muriate of ammonia.

Mr. Cruikshank has shown that oxymuriatic acid and hydrogen, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water; and MM. Gay Lussac and Thenard have stated that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oxymuriatic acid gas and hydrogen. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapor, and a condensation of from  $\frac{1}{10}$  to  $\frac{1}{20}$  of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oxymuriatic acid and the hydrogen by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures; but I have never been able to avoid a slight condensation; though, in proportion as the gases were free from oxygen or water, this condensation diminished.

I mixed together sulphuretted hydrogen in a high degree of purity and oxymuriatic acid gas, both dried, in equal volumes: in this instance the condensation was not  $\frac{1}{20}$ ; sulphur, which seemed to contain a little oxymuriatic acid, was formed on the sides of the vessel; no vapor was deposited; and the residual gas contained about  $\frac{19}{20}$  of muriatic acid gas, and the remainder was inflammable.

MM. Gay Lussac and Thenard have proved by a copious collection of instances, that in the usual cases where oxygen is procured from oxymuriatic acid, water is always present, and muriatic acid gas is formed: now, as it is shown that oxymuriatic acid gas is converted into muriatic acid gas by combining with hydrogen, it is scarcely possible to avoid the conclusion, that the oxygen is derived from the decomposition of water,



and, consequently, that the idea of the existence of water in muriatic acid gas is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxygen in oxymuriatic acid gas.

MM. Gay Lussac and Thenard indeed have stated an experiment, which they consider as proving that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious that in this case they formed the same compound as that produced by the action of oxymuriatic acid on lead; and in this process the muriatic acid must lose its hydrogen, and the lead its oxygen; which of course would form water: these able chemists, indeed, from the conclusion of their memoir, seem aware that such an explanation may be given, for they say that the oxymuriatic acid *may be considered* as a simple body.

I have repeated those experiments which led me first to suspect the existence of combined water in muriatic acid, with considerable care; I find that, when mercury is made to act upon 1 in volume of muriatic acid gas, by Voltaic electricity, all the acid disappears, calomel is formed, and about .5 of hydrogen evolved.

With potassium, in experiments made over very dry mercury, the quantity of hydrogen is always from nine to eleven, the volume of the muriatic acid gas used being 20.

And in some experiments made very carefully by my brother Mr. John Davy, on the decomposition of muriatic acid gas, by heated tin and zinc, hydrogen equal to about half its volume was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oxymuriatic gas, resulted.

It is evident from this series of observations, that Scheele's view (though obscured by terms derived from a vague and unfounded general theory) of the nature of the oxymuriatic and muriatic acids may be considered as an expression of facts; whilst the view adopted by the French school of chemistry, and which, till it is minutely examined, appears so beautiful

and satisfactory, rests, in the present state of our knowledge, upon hypothetical grounds.

When oxymuriatic acid is acted upon by nearly an equal volume of hydrogen, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted on by mercury, or any other metal, the oxymuriatic acid is attracted from the hydrogen, by the stronger affinity of the metal; and an oxymuriate, exactly similar to that formed by combustion, is produced.

The action of water upon those compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oxymuriatic acid with inflammable bases, may be easily explained, according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystallized mass is obtained, from which oxide of tin and muriate of ammonia can be procured by ammonia. In this case, oxygen may be conceived to be supplied to the tin, and hydrogen to the oxymuriatic acid.

The compound formed by burning phosphorus in oxymuriatic acid is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids; oxygen, it may be supposed, is furnished to the phosphorus to form phosphoric acid, hydrogen to the oxymuriatic acid to form common muriatic acid gas.

None of the combinations of the oxymuriatic acid with inflammable bodies can be decomposed by dry acids; and this seems to be the test which distinguishes the oxymuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash for instance, if M. Berthollet's estimation of its composition approaches towards accuracy, when ignited, is a compound of oxymuriatic acid with potassium: muriate of ammonia is a compound of muriatic acid gas and ammonia; and when acted on by potassium, is decomposed: the oxymuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hydrogen are set free.

The vivid combustion of bodies in oxymuriatic acid gas, at first view, appears a reason why oxygen should be admitted in



it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids, become ignited during their mutual agency; and such an effect might be expected in an operation so rapid, as that of oxymuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oxymuriatic acid consists of an acid basis united to oxygen, exists in the general analogy of the compounds of oxymuriatic acid, and metals, to the common neutral salts: but this analogy, when strictly investigated, will be found to be very indistinct; and even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water, and the metals of bases with hydrogen; and that, in the case of the action of oxymuriatic acid and metals, metal furnishes hydrogen to form muriatic acid, and a basis to produce the neutral combination.

That the quantity of hydrogen evolved during the decomposition of muriatic acid gas by metals, is the same that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hydrogen with oxymuriatic acid, one quantity must always be separated. Hydrogen is disengaged from its oxymuriatic combination, by a metal, in the same manner as one metal is disengaged by another from similar combinations; and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hydrogen is that which seems to adhere to oxymuriatic acid with the least force.

I have caused strong explosions from an electrical jar to pass through oxymuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change.

I electrized the oxymuriates of phosphorus and sulphur for some hours, by the power of the Voltaic apparatus of 1000 double plates: no gas separated, but a minute quantity of hydrogen, which I am inclined to attribute to the presence of

moisture in the apparatus employed; for I once obtained hydrogen from Libavius's liquor by a similar operation: but I have ascertained that this was owing to the decomposition of water adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled, there was no production of any permanent elastic matter.

As there are no experimental evidences of the existence of oxygen in oxymuriatic acid gas, a natural question arises concerning the nature of these compounds, in which the muriatic acid has been supposed to exist, combined with much more oxygen than oxymuriatic acid, in the state in which it has been named, by Mr. Chenevix, hyperoxygenized muriatic acid.

Can the oxymuriatic *acid* combine either with oxygen or hydrogen, and form with each of them an acid compound; of which that with hydrogen has the strongest, and that with oxygen the weakest affinity for bases? for the able chemist to whom I have just referred, conceives that hyperoxymuriates are decomposed by muriatic acid. Or, is hyperoxymuriatic acid, the basis of all this class of bodies, the most simple form of this species of matter?

The phenomena of the composition and decomposition of the hyperoxymuriates may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyperoxygenized, from hyperoxymuriate of potash by various modes, but uniformly without success. By distilling the salt with dry boracic acid, though a little oxymuriatic acid is generated, yet oxygen is the chief gaseous product, and a muriate of potash not decomposable is produced.

The distillation of the orange-colored fluid, produced by dissolving hyperoxymuriate of potash in sulphuric acid, affords only oxygen in great excess, and oxymuriatic acid.



When solutions of muriates or muriatic acid are electrized in the Voltaic circuit, oxymuriatic acid is evolved at the positive surface, and hydrogen at the negative surface. When a solution of oxymuriatic acid in water is electrized, oxymuriatic acid and oxygen appear\* at the positive surface, and hydrogen at the negative surface; facts which are certainly unfavourable to the idea of the existence of hyperoxygenized muriatic acid, whether it be imagined a compound of oxymuriatic acid with oxygen, or the basis of oxymuriatic acid.

If the facts respecting the hyperoxymuriate of potash, indeed, be closely reasoned upon, it must be regarded as nothing more than as a triple compound of oxymuriatic acid, potassium and oxygen. We have no right to assume the existence of any peculiar acid in it, or of a considerable portion of combined water; and it is perhaps more conformable to the analogy of chemistry, to suppose the large quantity of oxygen combined with the potassium, which we know has an intense affinity for oxygen, and which, from some experiments, I am inclined to believe, is capable of combining directly with more oxygen than exists in potash, than with the oxymuriatic acid, which, as far as is known, has no affinity for that substance.

It is generally supposed that a mixture of oxymuriatic acid and hyperoxymuriatic acid is disengaged when hyperoxymuriate of potash is decomposed by common muriatic acid† but I am satisfied from several trials, that the gas procured in this way, when not mixed with oxygen, unites to the same quantity of hydrogen,‡ as common oxymuriatic acid gas from manganese; and I find, by a careful examination, that the gas dis-

\* The quantity of oxymuriatic acid in the aqueous solution is so small that the principal products must be referred to the decomposition of water. This happens in other instances; the water only is decomposed in dilute solutions of nitric and sulphuric acids.

† If hyperoxymuriate of potash be decomposed by nitric or sulphuric acid, it affords oxymuriatic acid and oxygen. If it be acted upon by muriatic acid, it affords a large quantity of oxymuriatic acid gas only. In this last case, the phenomenon seems merely to depend upon the decomposition of the muriatic acid gas, by the oxygen, loosely combined in the salt.

‡ This likewise appears from Mr. Cruikshank's experiments. See Nicholson's Journal, vol. v. 4to, p. 206.

engaged during the solution of platina, in a mixture of nitric and muriatic acids, which has been regarded as hyperoxymuriatic acid, but which I stated some years ago to possess the properties of oxymuriatic acid gas,\* is actually that body, owing its peculiar color to a small quantity of nitromuriatic vapor suspended in it, and from which it is easily freed by washing.

Few substances, perhaps, have less claim to be considered as acid, than oxymuriatic acid. As yet we have no right to say that it has been decomposed; and as its tendency of combination is with pure inflammable matters, it may possibly belong to the same class of bodies as oxygen.

May it not in fact be a *peculiar* acidifying and dissolving principle, forming compounds with combustible bodies, analogous to acids containing oxygen or oxides, in their properties and powers of combination; but differing from them, in being for the most part decomposable by water. On this idea muriatic acid may be considered as having hydrogen for its basis, and oxymuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oxymuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oxymuriatic acid, may be regarded as analogous bodies. The combinations of oxymuriatic acid with lead, silver, mercury, potassium, and sodium, in this view would be considered as a class of bodies related more to oxides than acids, in their powers of attraction.

It is needless to take up the time of this learned society by dwelling upon the imperfection of the modern nomenclature

\* The platina, I find by several experiments made with great care, has no share in producing the evolution of this gas. It is formed during the production of aqua regia. The hydrogen of the muriatic acid attracts oxygen from the nitric acid. Oxymuriatic acid gas is set free, and nitrous gas remains in the solution, and gives it a deep red color. Nitrous acid and muriatic acid produce no oxymuriatic acid gas. Platina, during its solution in perfectly formed aqua regia, gives only nitrous gas and nitrous vapor; and I find, that rather more oxymuriatic acid gas is produced, by heating together equal quantities of nitric acid of 1.45, and muriatic acid of 1.18, when they are not in contact with platina, than when exposed to that metal. The oxymuriatic acid gas produced from muriatic acid, by nitric acid, I find combines with about an equal volume of hydrogen by detonation.



of these substances. It is in many cases connected with false ideas of their nature and composition; and in a more advanced state of the inquiry, it will be necessary for the progress of science, that it should undergo material alterations.

It is extremely probable that there are many combinations of the oxymuriatic acid with inflammable bodies which have not been yet investigated. With phosphorus it seems capable of combining in at least three proportions; the phosphuretted muriatic acid of Gay Lussac and Thenard is the compound containing the maximum of phosphorus. The crystalline phosphoric sublimate, and the liquor formed by the combustion of phosphorus in oxymuriatic acid gas, disengage no phosphorus by the action of water; the sublimate, as I have already mentioned, affords phosphoric and muriatic acid; and the liquid I believe only phosphorous acid and muriatic acid.

The sublimate from the boracic basis gives, I believe, only boracic and muriatic acid, and may be regarded as boracium acidified by oxymuriatic acid.

It is evident, that whenever an oxymuriatic combination is decomposed by water, the oxide or acid or alkali or oxidated body formed must be in the same proportion as the muriatic acid gas, as the oxygen and hydrogen must bear the same relation to each other; and experiments upon these compounds will probably afford simple modes of ascertaining the proportions of the elements, in the different oxides, acids, and alkaline earths.

If, according to the ingenious idea of Mr. Dalton, hydrogen be considered as one in weight, in the proportion it exists in water, then oxygen will be nearly 7.5; and assuming that potash is composed of one proportion of oxygen, and one of potassium, then potash will be 48, and potassium\* about 40.5; and from an experiment which I have detailed in the last Bakerian lecture, on the combustion of potassium in muriatic acid gas, oxymuriatic acid will be represented by 32.9, and muriatic acid gas, of course, by 33.9; and this estimation agrees with the specific gravity of oxymuriatic acid gas, and muriatic acid

\* Supposing potash to contain nearly 15.6 per cent. of oxygen.

gas. From my experiments, 100 cubical inches of oxymuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74.5 grains; whereas by estimation they should weigh 74.6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38.4 grains.

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide or of acid it would furnish by the action of water, and consequently the quantity of oxygen with which the inflammable matter will combine.\*

In considering the dry muriates, as compounds of oxymuriatic acid and inflammable bodies; the argument that I have used in the last Bakerian lecture, to show that potassium does not form hydrate of potash by combustion, is considerably strengthened; for from the quantity of oxymuriatic acid the metal requires to produce a muriate, it seems to be shown that

\* I have stated in the last Bakerian lecture, that during the decomposition of the amalgam from ammonia, one in volume of hydrogen to two of ammonia is evolved: it is remarkable, that whatever theory of the nature of this extraordinary compound be adopted, there will be a happy coincidence as to definite proportions. If it be supposed that the hydrogen arises from the decomposition of water; then the oxygen that must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hydrogen, in an equal volume of muriatic acid; or if it be said that ammonium is a compound of two of ammonia and one of hydrogen in volume, then equal volumes of muriatic acid gas and ammonia will produce the same compound as oxymuriatic acid and ammonium, supposing they could be immediately combined. I once thought that the phenomena of metallization might be explained according to a modified phlogistic theory, by supposing three different classes of metallic bodies: First, The metal of ammonia, in which hydrogen was so loosely combined as to be separable with great ease, and which, in consequence of the small affinity of the basis for water, it had little tendency to combine with oxygen. The second, the metals of the alkalies and alkaline earths, in which the hydrogen was more firmly combined, but in combustion, forming water capable of being separated from the basis. And, thirdly, the metals of the earths and common metals, in which the hydrogen was more intimately combined; producing by union with oxygen, water not separable by any new attractions. The phenomena of the action of potassium and sodium upon muriatic acid, referred to in the text, seem however to overturn these speculations so far as they concern the metals from the fixed alkalies.



it is the simplest known form of the alkaline matter. This I think approaches to an *experimentum crucis*. Potash made by alcohol, and that has been heated to redness, appears to be a hydrate of potash, whilst the potash formed by the combustion of potassium must be considered as a pure metallic oxide, which requires about 19 per cent. of water to convert into a hydrate.

Amongst all the known combustible bodies, charcoal is the only one which does not combine directly with oxymuriatic acid gas; and yet there is reason for believing that this combination may be formed by the intermedium of hydrogen. I am inclined to consider the only substance produced by the action of oxymuriatic acid gas, and olefiant gas, as a ternary compound of these bodies; for they combine nearly in equal volumes; and I find that, by the action of potassium upon the oil so produced, muriate of potash is formed, and gaseous matter, which I have not yet been able to collect in sufficient quantity to decide upon its nature, is formed. Artificial camphor, and muriatic ether, as is probable from the ingenious experiments of M. Gehlen and M. Thenard, must be combinations of a similar kind, one probably with more hydrogen, and the other with more carbon.

One of the greatest problems in economical chemistry, is the decomposition of the muriates of soda and potash. The solution of this problem will, perhaps, be facilitated by these new views. The affinity of potassium and sodium for oxymuriatic acid is very strong; but so likewise is their attraction for oxygen, and the affinity of their oxides for water. The affinities of oxymuriatic acid gas for hydrogen, and of muriatic acid gas for water, are likewise of a powerful kind. Water, therefore, should be present in all cases, when it is intended to attempt to produce alkali. It is not difficult after these views to explain the decomposition of common salt, by aluminous or siliceous substances, which, as it has been long known, act only when they contain water. In these cases the sodium may be conceived to combine with the oxygen of the water and with the earth, to form a vitreous compound; and the oxymuriatic acid to unite with the hydrogen of the water, forming muriatic acid gas.

It is also easy, according to these new ideas, to explain the decomposition of salt by moistened litharge, the theory of which has so much perplexed the most acute chemists. It may be conceived to be an instance of compound affinity: the oxymuriatic acid is attracted by the lead, and the sodium combines with the oxygen of the litharge and with water to form hydrate of soda, which gradually attracts carbonic acid from the air.

As iron has a strong affinity for oxymuriatic acid, I attempted to procure soda by passing steam over a mixture of iron filings, and muriate of soda intensely heated: and in this way I succeeded in decomposing some of the salt: hydrogen came over: a little hydrate of soda was formed; and muriate of iron was produced.

It does not seem improbable, supposing the views that have been developed accurate, that by complex affinities, even potassium and sodium in their metallic form may be procured from their oxymuriatic combinations: for this purpose the oxymuriatic acid should be attracted by one substance, and the alkaline metals by another; and such bodies should be selected for the experiment, as would produce compounds differing considerably in degree of volatility.

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the Society to some of the theoretical relations of the facts noticed in the preceding pages.

That a body principally composed of oxymuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phenomenon of a perfectly new kind. Three bodies, two of which are permanent gases, and the other of which is considerably volatile, form in this instance a substance neither fusible nor volatile, at a white heat. It could not have been expected that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oxymuriatic acid, would have appeared incredible, according to all the existing analogies of chemistry. The experiments on which these conclusions are founded, are, however,



uniform in their results: and it is easy to repeat them. They seem to show, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oxymuriatic acid, phosphorus, and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common reagents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary, may be reduced into simpler forms of matter? and that an intense attraction, and an equilibrium of attraction, may give to a compound, containing several constituents, that refractory character, which is generally attributed to unity of constitution, or to the homogeneous nature of its parts?

Besides the compound of the phosphoric sublimate and ammonia, and the other analogous compounds which have been referred to, it is probable that other compounds of like nature may be formed of the oxides, alkalies, and earths, with the oxymuriatic combinations, or of the oxymuriatic compounds with each other; and should this be the case, the more refined analogies of chemical philosophy will be extended by these new, and, as it would seem at first view, contradictory facts. For if, as I have said, oxymuriatic acid gas be referred to the same class of bodies as oxygen gas, then, as oxygen is not an acid, but forms acids by combining with certain inflammable bodies, so oxymuriatic acid, by uniting to similar substances, may be conceived to form either acids, which is the case when it combines with hydrogen, or compounds like acids or oxides, capable of forming neutral combinations, as in the instances of the oxymuriates of phosphorus and tin.

Like oxygen, oxymuriatic acid is attracted by the positive surface in Voltaic combinations; and on the hypothesis of the connexion of chemical attraction with electrical powers, all its energies of combination correspond with those of a body supposed to be negative in a high degree.

And in most of its compounds, except those containing the alkaline metals, which may be conceived in the highest degree positive, and the metals with which it forms insoluble compounds, it seems still to retain its negative character.

I shall occupy the time of the society for a few minutes longer only, for the purpose of detailing a few observations connected with the Bakerian lectures, delivered in the two last years; particularly those parts of them relating to sulphur and phosphorus, which new and more minute inquiries have enabled me to correct or extend.

I have already mentioned that there are considerable differences in the results of experiments, made on the action of potassium, on sulphur and phosphorus, and their combinations with hydrogen, according to different circumstances of the process. I shall now refer to such of these circumstances as I have been able fully to investigate.

The able researches of Dr. Thomson have shown that sulphur, in its usual state, contains small quantities of acid matter; and though, in my first experiments, I conceived that by employing crystallized native sulphur, which had been recently sublimed in nitrogen, I should avoid the presence of any foreign matter, yet I am inclined to believe that this is not the case; for by subliming some similar sulphur in nitrogen, I find that litmus paper placed in the upper part of the retort is slightly reddened.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid; sulphuretted hydrogen likewise is soluble in muriatic acid: and this circumstance led me to underrate the quantity of sulphuretted hydrogen given off in experiments of this kind.\*

\* This circumstance has been pointed out by MM. Gay Lussac and Thenard, in a paper printed in the *Journal de Physique* for December, in which these gentlemen endeavour to show that, whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it pro-



In acting upon sulphuretted hydrogen by potassium in my early experiments, I used large quantities of the gas and of the metal; and in these cases I have reason to believe that the violence of the combustion occasioned the decomposition of a considerable quantity of the gas; and, in consequence, led me to form erroneous conclusions concerning the nature of this curious operation.

In all late experiments in which sulphur or sulphuretted hydrogen was concerned, I have used muriatic acid saturated with sulphuretted hydrogen over mercury. I have employed sulphur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green-glass, or plate-glass lined with sulphur and filled with very pure nitrogen or hydrogen. In making potassium act upon sulphuretted hydrogen, I have employed the gas only in the quantities of from one to three cubical inches, and have made the combination in narrow curved tubes of green-glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is a sufficient correspondence between them to enable me to form conclusions, which I may venture to say cannot be far from the truth.

When one grain of potassium, which would give by the action of water about one cubical inch and  $\frac{1}{16}$  of hydrogen is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light, and from  $\frac{1}{16}$  to  $\frac{1}{8}$  of a cubical inch of sulphuretted hydrogen is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hydrogen, affords from  $\frac{9}{16}$  to  $\frac{11}{16}$  of a cubical inch of pure sulphuretted hydrogen.

When more sulphur is used so as to be from twice to ten times the weight of the potassium, the quantity of sulphuretted hydrogen evolved by the action of the acid, is from  $\frac{7}{16}$  to  $\frac{9}{16}$ ; but if heat be applied to the combination, so as to drive

duces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same journal in which their memoir has appeared) offered some observations on their inquiries.

off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used; and I am inclined to believe, from the phenomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one proportion, in which the metal is to the sulphur nearly as three to one in weight; and in which the quantities are such that the compound burns into neutral sulphate of potash.

When a grain of potassium is made to act upon about 1.1 cubical inches of sulphuretted hydrogen, all the hydrogen is set free, and a sulphuret of potassium containing one fourth of sulphur is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

When sulphuretted hydrogen is employed in larger quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hydrogen disengaged, and a compound of sulphuretted hydrogen and sulphuret of potash is formed, which gives sulphuretted hydrogen by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

From a number of experiments I am inclined to believe that potassium and phosphorus, in whatever quantities they are heated together, combine only in one proportion, a grain of potassium requiring about  $\frac{3}{10}$  of a grain of phosphorus to form a phosphuret; which when acted upon by muriatic acid, produces from  $\frac{8}{10}$  to  $\frac{10}{10}$  of a cubical inch of phosphuretted hydrogen.

Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hydrogen, and sets free rather more than four cubical inches of hydrogen; and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

If, according to Mr. Dalton's ideas of proportion, the quantity in which sulphur enters into its combination were to be deduced from its union with potassium, in which it seems to form about one fourth the weight of the compound, the number representing it would be 13.5. I have lately weighed sulphuret-



ted hydrogen and sulphurous acid gas, with great care: the specific gravity of the first at mean temperature and pressure, from my experiments, is 10645, which differs very little from the estimation of Mr. Kirwan: that of sulphurous acid gas I find is 20967. Sulphuretted hydrogen, as I have shown, contains an equal volume of hydrogen; and on this datum the number representing sulphur is 13·4. I have never been able to burn sulphur in oxygen without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphurous acid from 100 of oxygen in volume; from which I am inclined to believe, that sulphurous acid consists of sulphur dissolved in an equal volume of oxygen; which would give the number as 13·7\* nearly considering the acid gas as containing one proportion of sulphur, and two of oxygen; and these estimations do not differ from each other materially.

I have made several experiments on the combustion of phosphorus in oxygen gas. From the most accurate, I am inclined to conclude that 25 of phosphorus absorbed in combustion about 34 of oxygen in weight: and considering phosphoric acid as composed of three portions of oxygen and one of phosphorus, the number representing phosphorus, will be about 16·5, which is not very remote from the number that may be deduced from the composition of phosphuret of potassium.

The numbers which represent the proportions in which sulphur and phosphorus unite with other bodies, are such, as do not exclude the existence of combined portions of oxygen and

\* The estimation from the composition of sulphuretted hydrogen, must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian lecture, I have estimated the specific gravity of sulphuretted hydrogen at 35 grains the 100 cubical inches, which was not far from the mean between the estimations of Mr. Kirwan and Mr. Thenard. According to this last experiment, sulphuretted hydrogen is composed of one proportion of hydrogen, represented by 1, and one of sulphur represented by 13·4.

hydrogen in their constitution; but it may be questioned, whether the opinion which I formed, that the inflammable gas disengaged from them by electricity, is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian lecture is capable of forming a solid hydruret: and a part of the sulphur distilled from iron pyrites is usually of a soft consistence, and emits the smell of sulphuretted hydrogen, and probably contains that body. It is not unlikely, that in all cases, phosphorus and sulphur contain small quantities of the hydrurets of phosphorus and sulphur; and the production of a minute portion of sulphuric acid in the slow combustion of sulphur, is probably connected with the production of water. Though the pure oxides of sulphur and phosphorus have never been obtained, yet from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe, that they sometimes exist in minute quantities in common phosphorus and sulphur, and with hydrogen give to them their variable properties.

The colors of different specimens of phosphorus, as well as of sulphur, differ considerably; the red color of phosphorus as it is commonly prepared, is probably owing to a slight mixture of oxide. Common roll sulphur is of a very pale yellow, the Sicilian sulphur of an orange color, and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish-green color. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxygen in Sicilian sulphur, which is probably owing to the presence of oxide of sulphur, which may give rise to sulphuric acid in distillation, or to sulphuric acid itself.

Conceiving that, if definite proportions of oxygen and hydrogen existed in sulphur and phosphorus, they ought to be manifested in the agency of oxymuriatic acid gas on these bodies, I made some experiments on the results of these operations. In the first trial, on the combination of sulphur with oxymuriatic acid gas, I employed five grains of roll sulphur, and admitted the gas into the exhausted retort, from a vessel



in which it had been in contact with warm water: in this case more than a half a cubical inch of oxygen gas, and nearly two cubical inches of muriatic acid gas, were produced. Suspecting in this instance, that aqueous vapor had been decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime: in this case, though Sicilian sulphur was used, no oxygen gas was evolved; and not a half a cubical inch of muriatic acid; the quantity was the same as in the last experiment; and it was found, that between 16 and 17 cubical inches of oxymuriatic acid gas disappeared; the whole of the sulphur was sublimed in the gas, and the liquor formed was of a tawny orange color.

No oxygen was expelled during the combustion of phosphorus in oxymuriatic acid gas, nor could I ascertain that any muriatic acid had been formed; three grains of phosphorus were entirely converted into sublimate, by the absorption of about 23 cubical inches and a half of the gas.

It would seem from these quantities, that the sulphuretted liquor formed by subliming sulphur in oxymuriatic acid gas, consists of one proportion of sulphur, represented by 13·5, and one of oxymuriatic gas represented by 32·9, and that the phosphoric sublimate must be composed of three portions of oxymuriatic gas, represented by 98·7 and one of phosphorus represented by 16·5.

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*Cases illustrating the Effects of Oil of Turpentine in expelling the Tape-worm.*

By JOHN COAKLEY LETTSOM, M. D. and President of the Medical Society.

From the Philosophical Magazine, vol. xxxvi.

CASE I.

EARLY in September 1809, I was consulted by J. P. esq., about thirty-five years of age, on account of an uneasiness in the abdomen, with dyspepsia, which were supposed to originate from tania, or tape-worm, as small portions of it had occasionally been evacuated by the rectum.

I prescribed a course of the male fern, with occasional cathartics, as recommended by Madame Noufflet. In this plan he persevered for the space of three months, in which period he discharged, at two different times, about eight yards of the tænia. In April 1810, he again applied to me, in consequence of labouring under his former complaints; adding that he imagined, from the long use of the plant I had recommended, his pains, and particularly the dyspepsia and general debility, had increased.

At this time I ordered the oleum terebinthinæ rectificatum, in a dose of nine drachms by weight, and after it a little honey to remove the heat and unpleasant taste it might occasion. In a week after taking the oil, he called upon me agreeably to my request, and gave me the following information: That as far as he could judge, in swallowing this medicine, it occasioned less heat than would have resulted from taking the same quantity of brandy, or other spirit; and that the taste and heat it produced were soon removed by the honey.

In about three hours after taking this dose, a laxative motion was produced, without any discharge of tænia: but soon afterwards, with the second stool, more than four yards of the worm were discharged, and also a quantity of matter, resembling, as the patient expressed it, the substance and skins of the tænia. On the surface of this evacuation he noticed the oil floating, together with the tænia and the substance mentioned. It produced little or no pain, and certainly much less than the purgative he had taken after the use of the male fern. The subsequent motions contained no tænia, nor any of the substance before noticed. He experienced no pain or heat in the urinary passages, though the urine continued to impart a terebinthinate smell for three or four successive days.

My patient has since remained in perfect health, enjoying a degree of comfort to which he had been a stranger for the preceding half year.

From this and other instances, I am induced to conclude that the best method of taking the oil is without any admixture; and that the dose of nine drachms occasions very little inconvenience; and farther, that this quantity, perhaps owing



to its quick purgative effect, excites no irritation in the urinary passages, although it imparts its peculiar smell to the urine.

I do not recollect that it has been heretofore observed, that the oil has been evacuated in its original state. It might hence be inferred, that it is most efficacious when exhibited uncombined, in which state it is not attended with any particular inconvenience.

It is well known that tænia may exist in a healthy state of the system; and that hence its presence cannot be accurately ascertained by any other circumstance than the actual discharge of portions of the worm itself. Sometimes, indeed, there is felt a heavy pain in the epigastrium, attended with dyspepsia and emaciation; but these are not pathognomonic symptoms, as they may arise from other causes.

## CASE II.

By THOMAS HANCOCK, Physician to the Queensbury Dispensary.

I have used the ol. terebinth. in only one case of tænia. Jane Woodward, a poor woman, about 45 years of age, first applied to me at the London Electrical Dispensary, some months ago. She had been for more than seven years afflicted in a very distressing manner with this complaint, and was four times a patient in different hospitals, where by the use of active remedies, she obtained temporary relief from pain, and frequently voided large portions of the tænia per anum. So soon as she had recovered a little strength, by indulging her appetite, after the violent operation of purgatives in these hospitals, her abdomen began to increase considerably in size, and small detached portions of tænia, about an inch or more in length, apparently endowed with life, continued to pass at times through the rectum; so that she was prevented from earning her bread, by this very distressing circumstance. She had generally recourse to purgatives on these occasions, and their operation had regularly the effect of reducing the size of the abdomen; but her disease continued. I may also observe, that, after these courses of medicine, she had less of rumbling in the intestines, and felt less pain, than when she freely indulged her appetite; for then, to use her own expressions, "the worms appeared to gain strength," according to the increase of her own strength.

About two weeks after the application of electrical sparks to the abdomen, she discharged a portion of tænia, seven yards in length, without any appearance of head, which lived in cold water nearly three hours after its expulsion. Mr. Chamberlaine informs me he has known the tænia live nearly as long in water which was much above the temperature of the human body; a sufficient proof of the extraordinary tenacity of life in this animal.

Electricity was continued for some weeks longer; but as her pains also continued, and no more of the worm came away, my friend Mr. Chamberlaine kindly offered to try the effect of his electuary of the *dolichos pruriens*. The patient took this at first without, but afterwards with, the *scobs stanni* in large quantities, and for a considerable time: but though, as she asserted, these medicines, more than any she had ever taken, relieved her sufferings, they produced no discharge of tænia.

I now heard of the *ol. tereb.* having been administered in this complaint, and resolved to give it a fair trial, especially as my patient was herself very anxious to use any remedy that promised the slightest probability of success. I may premise, that her abdomen was enlarged as formerly, her stools slimy, and, in short, all her symptoms indicated that she had still large portions of tænia in her intestines. I ordered her at first small doses of this oil, viz. two drachms twice a day mixed with treacle to disguise its taste. This produced no other effect than an increase of pain and uneasiness, and particularly on going to stool, as if it irritated the rectum. The dose was now increased to half an ounce, at longer intervals. The first dose in this quantity, which she took without treacle, produced a little sickness and confusion of ideas, and afterwards operated as a purge. She complained of no uneasiness whatever in the urinary organs. After these doses, she passed such a quantity of slimy mucus, with such relief in all her painful symptoms, that she earnestly entreated I would allow her to take a double dose. The quantity of an ounce, which she now took, always produced a great degree of giddiness, as if she was intoxicated, which came on shortly after taking it and con-



tinued for an hour or more, until the violent cathartic effect which followed, removed it.

Although this medicine was repeated, after this manner, every two or three days for a fortnight or more, by her own particular desire, there was no appearance of tania in her stools. I could not, however, but observe, that the mucus which was discharged so abundantly by the operation of the ol. tereb. sometimes exhibited the appearance of white films, as if the substance of the worm had been broken down. She took the very large dose of an ounce and half two or three times, after the medicine began to lose its effect, with results similar to those I have described. In short, by her own account, violent purging was the only thing that relieved her; and all kinds of strengthening remedies, as well as nourishing diet, uniformly increased her sufferings, so that she abstained from food when her appetite craved it, in order to avoid the anticipated pain.

I have since heard that she went into the London hospital, and had again taken the ol. tereb.; for I strongly advised her to discontinue its use some time before she left the Finsbury Dispensary, having lost all hopes of its ultimately curing her.

### CASE III.

By SAMUEL FOTHERGILL, M. D. F. M. S. Physician to the Western Dispensary.

A soldier aged about 40, applied to the Western Dispensary, the 28th of October, 1809. He stated that he had been subject to tape-worm during the last four years, previously to which he had served with the army in Egypt, and attributed the origin of his complaint to the badness of the water which he drank in that country. He is now a private in the Middlesex militia. He complains of gnawing pains in the abdomen, irregular appetite, debility, and anxiety. He is somewhat emaciated, and his complexion is rather sallow. Whilst with his regiment, he had occasionally taken, by order of the surgeon, a variety of worm medicines, and small pieces of tape-worm were passed at times; they even sometimes came away alive, without medicine having been taken, and without a stool.

I directed him to take pulv. scammon. cum calomel. ℞j.

every third morning. Two doses operated freely, but only a few very short pieces of tape-worm were brought away. I now directed him to take half an ounce of the oil of turpentine. He took it as ordered, November 9, in a little tea, sweetened with honey. In a quarter of an hour he was seized with retching, and in the course of the day passed four copious stools, in one of which was a tape-worm of several yards in length. The portion which the patient brought me I found measured four yards; he threw the smaller pieces away; but thought that altogether the length might be ten yards. The worm was dead, and had a livid appearance: the patient remarked that the pieces which formerly passed from him were of a whiter color and brighter aspect.

The dose of the medicine was increased to six drachms, and was repeated twice a week for the space of a month. During the first fortnight small pieces of worm continued to pass away, both after taking the medicine and at other times; but in the second fortnight the stools were natural, and contained no vestige of tænia. The remedy was consequently discontinued; and the man called some weeks afterwards to acquaint me that he had remained entirely free from all symptoms of his complaint, and had regained his strength and cheerfulness.

He was generally a little sick after taking the medicine, and for a day or two was affected with a severe pain in the back part of his head, but complained of no other unpleasant effects from its use.

#### CASE IV. and V.

By GEORGE BIRKBECK, M. D. F. M. S. Physician to the General Dispensary.

Dr. Birkbeck stated to the Society that he had administered the oil of turpentine, successfully, to two middle-aged females who had long been troubled with the tape-worm.

In the first case half an ounce was given: no unpleasant sensation occurred whilst it was swallowed, but considerable confusion of ideas and vertigo, with a slight degree of nausea, were soon produced. In a short time a discharge from the bowels took place; this was quickly followed by another, with which more than four yards of the worm were evacuated. The



patient, in consequence of the frequent spontaneous escape of small portions of tania, and the expulsion of a larger quantity about twelve months before by an active purgative medicine, had an opportunity of comparing the ordinary appearance of the worm with that which it now presented. Instead of being whitish, smooth, full, and in motion, she represented it to be dark-colored, shrivelled, filmy, and lifeless. A second dose of the oil did not expel any more of the worm, nor, when he last saw her, about three months afterwards, had it again appeared. In that interval she had not been disturbed by any of the unpleasant feelings to which she before was subjected.

Considerable derangement of the general health and great pain in the pit of the stomach were produced by the tape-worm, in the second case in which the oil of turpentine was employed. Although one tea-spoonful only was introduced, sickness and acute pain followed: this dose was repeated several successive mornings, always with the same immediate effects; but occasionally it was succeeded by the expulsion of large portions of the worm. The worm was represented to have the appearance before noticed. The patient had sufficient resolution and confidence to continue for some time the use of the medicine, and at length became free, not only from any further appearance of tania in the stools, but likewise from all those sensations which had so long denoted its presence in the intestines.

#### CASE VI.

By JAMES SANER, Surgeon, F. M. S.

A woman, about 40 years of age, came to me in May last, very much agitated, having just voided about six or eight feet of tape-worm. She told me, that pieces had come away for the last seven years whenever she took a dose of jalap, which she had done that morning. She never found any thing to relieve her so much as the jalap, though she had taken a great deal of medicine from respectable practitioners, and had also been under the care of a noted empiric for two years.

I thought this a good opportunity for trying the ol. tereb. rectificat. I therefore gave her one ounce with an equal quantity of syrup of saffron. In less than two hours she returned to

me with about eight feet of the worm, with the head attached. She was very much gratified by this, as she had been told to look for the small black head. The medicine did not produce any unpleasant sensation; merely a slight degree of nausea, a giddiness as if intoxicated, and a frequent desire to void urine, though without pain.

The day after, she complained of a feeling of emptiness in the stomach. I gave her the decoct. cinchonæ for a few days, which completely removed the sensation, and she has remained perfectly well ever since.

P. S. The woman informed me she used to eat raw meat formerly, as it seemed to ease her stomach more than any thing else; but since she voided the worm, she has had no craving for it.

## CASE VII.

BY THE SAME.

Since communicating the above, I am sorry to say I have had a case of tænia, where the ol. tereb. rect. has not so completely answered my expectation.

Being very sanguine in my opinion of it, in consequence of my former success, I mentioned the case to a relation of mine, who informed me he knew a labouring mechanic (a Russian) who had voided large pieces of tape-worm for a number of years. He persuaded this man to visit me, and I gave him the same dose I had given my former patient. It brought away a very large quantity, but so very soft that I could not measure it. As I could not perceive any thing like the head of the worm, I advised him to repeat the dose in a few days, which he very readily complied with, as he had suffered very little from the first.

I gave him the same quantity as before (viz. ol. tereb. rect. et syr. croci  $\text{āā} \frac{3}{4}$  j.) This produced violent retchings, tenesmus, strangury, and great pain in the back; the urine was also a little tinged with blood. The strangury and tenesmus continued nearly a week, and the patient was not able to work for several days after. As he had not voided any portion of worm with the last dose, I concluded that he was quite well, but re-



quested he would call on me again in about two months. He called last week, and I advised him to try his old remedy (a drachm of jalap), which had its usual effect, in bringing away a large quantity of the worm. I fear I shall not be able to induce him again to try the ol. tereb., from the severe symptoms which it produced when he last used it.

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*Extracts from an Essay towards an Inquiry how far the Effluvia from dead Animal Bodies, passing through the natural process of Putrefaction, are efficient in the production of Malignant Pestilential Fevers; and how far such Effluvia are capable "of exciting a Putrefactive Emotion in all other" living "Animal Substances exposed to their action?"*

By C. CHISHOLM, M. D. F. R. S. &c. &c.

From the Edinburgh Medical and Surgical Journal, for October 1810.

THERE are grounds of belief, that even the concentration of the miasms of putrid animal substances, does not give rise to fever, and seldom, if ever, to disease of any description. The following facts certainly militate against a contrary conclusion.

1. In the neighbourhood of Bitton, in Gloucestershire, about a mile from Willsbridge, which was my residence for nearly four years, there is what is called, "a bone manufactory," in which animal bones, after the extraction of their medullary oil by boiling, are distilled, and yield the usual products, muriate of ammonia, and sulphate of soda. From this manufactory, a factor of the most offensive nauseating nature proceeds, and fills the atmosphere for nearly a mile around, diminishing in strength as it recedes from its source, and in proportion to its dilution or decomposition. The country is thickly inhabited, and near the manufacture itself is the village of Oldland, the population of which is very considerable; yet, in not one instance has this manufacture proved, in the smallest degree, injurious to health. I have frequently visited it with the most complete impunity. For several years the superintendent, Mr. Henderson, his wife, and family, lived in a house, having on one side, connected with it that in which the retorts are placed,

and on the other, that in which the bones are boiled; yet they had every appearance of health, and they assured me they enjoyed it. A more convenient house, on a neighbouring hill, becoming vacant, Mr. Henderson rented it for the accommodation of his family. Soon after they began to reside in it, they lost their health, and were, when I last saw them, much inclined to return to their old and stinking habitation. This exemption from disease in the manufactory of sal ammoniac, &c. has been noticed by Morveau and Chaptal. (*Edin. Med. Journ.* vol. ii. p. 295.)

2. Between Bristol and Hanham, on the banks of the Avon, is Conham, remarkable for nothing but its having been chosen for the site of an extensive manufactory for the conversion of the flesh of dead animals into a substance resembling spermaceti: a project which has been relinquished several years ago. This being also not very distant from Willsbridge, I made a good deal of inquiry into the result, as far as it affected the health of those immediately engaged in the process, and of the inhabitants of its thickly peopled neighbourhood. The foreman or superintendent, Richard Bolston, residing now at Jeffries-hill near Hanham, has been my principal informant: and his account was confirmed by that of Mr. Thomas Pearsall of Willsbridge, and other respectable persons. Bolston was two years employed constantly in this business; and during that time resided in the midst of dead animal bodies, horses, asses and dogs, many of which were left to pass through the natural process of putrefaction. He had three labourers under him, and he declares that neither himself nor any of these men suffered from a moment's sickness, or indeed experienced the smallest inconvenience. Their business was to cut up the carcasses, to strip the muscular flesh from the bones, and to dispose of it first in boxes, perforated for the admission of water, which were afterwards laid in pits filled with water. The entrails and every part not useful to them, were left to putrefy on the surface. The pits prepared for the animal matter thus disposed were seven feet deep, and four broad and long, and each calculated to contain the flesh of fifty horses, besides asses and dogs. An idea may therefore be formed of the immense volume of pu-



trid animal effluvia, enveloping continually the persons of Bolston and the labourers, by being informed that there were six of these pits, and consequently 300 carcasses of horses, and as many of asses and dogs, exhaling in greater or less abundance their offensive miasms. Notwithstanding this, Bolston declares, that although the stench was offensive in the highest degree, yet he and those with him sustained no injury: and to this the inhabitants of the country around bear ample and angry testimony both in relation to Bolston and themselves.

3. Another remarkable fact, well known where the manufactory of refined sugar is extensively carried on: butchers preserve the blood of the slaughtered animals in open tubs, kept in close small shut up houses, sometimes for several weeks, until the quantity required is completed, or until there is a demand from the sugar-bakers for it. It is then, in a putrid state, conveyed through the public streets in carts or drays to the sugar-houses, emitting the most offensive effluvia, and extremely annoying to all those who pass it. It is seldom immediately used by the sugar-bakers, but kept by them in casks in a putrid state, filling the air of the manufactory, and frequently of the vicinity, with its putrid miasms, or what Galen and his followers would call *σπέρμα λοιμικὴ γενήσομενα*, the seeds of pestilence. But what is the result to the workmen, or to the inhabitants of the surrounding houses? nothing inimical to health. This fact exists constantly in the city of Bristol, where in general the streets are extremely narrow, and the houses excessively crowded and ill ventilated: and yet the harmless nature of these exhalations may be daily verified: I speak from my own observation, and the experience of the most respectable sugar-bakers. In summer it is more remarkable than in winter.

4. Mr. Newman, surgeon in Stokes Croft, Bristol, a gentleman of great worth and professional skill, procured for me from his friend Mr. Bevington and his brother Mr. Newman of Bermondsey in Southwark, the following interesting particulars respecting the leather-dressing business.

"I have just received your letter of the 20th inst. (January 1810), making inquiry respecting putrid, contagious, and low fevers, as affecting the workmen employed by leather-dressers,

to which I can give you a pretty clear reply. Our men are generally healthy, and the most so of the labouring poor: many have been in our service and knowledge fifteen and twenty years, and I do not recollect one case of the kind occurring (in our establishment) in London. The first process in dressing is to put the skins into a pit of water to soften them, which is often used two or three times, that is, for two or three parcels, before it is changed, until the stench is intolerable. After this process the skins are struck out over a beam, and hung up, side by side, as close as possible, in a small room excluded from external air, which we term a stove: in this state they remain until they heat and slime, so that we can pull off the wool. The process of putrefaction is here so rapid as to disengage large quantities of volatile ammoniac, affecting the eyes of strangers with tears, and their noses with the most offensive smell. Our men always pull the skins in the stove in cold weather from preference, and are occupied in it a whole day at a time without injury." Another gentleman, a brother of Mr. Newman's, concerned in the leather-dressing trade, but not in the same house, in Bermondsey, informs him "that so far from our workmen being unhealthy or particularly subject to fevers, the reverse is the fact; the men employed look generally robust and healthy. In a concern in this line of business of fifty years' standing, in which fifty men are constantly employed, the men have been uniformly healthy; and in this a circumstance is deserving of notice, viz. the men who work upon the raw skins, from which there is a constant and profuse exhalation of putrid steams, and those employed at the lime and tan-pits, are equally healthy." Mr. Newman the writer of the above, says there are about sixty leather-dressers' and tanners' yards in Bermondsey, and in them about 700 men are constantly employed.

It may perhaps be objected to this account that the business of leather-dressers in other countries had been represented as extremely unhealthy. Hippocrates is supposed to have meant something of this kind as the cause, when he mentions the case of a person, *Philiscus residing near the wall*, who died on the sixth day of a malignant fever. (Epidemic. Lib. i. s. 3.);



for anciently, and now indeed, offensive trades of this kind were carried on in the suburbs, *παρὰ τοὺς πόλιν* of cities. This was the case at Rome, beyond the Tiber, and some of the Latin poets have exercised their wit in allusions to it. It is highly probable, however, that the real cause naturally existed in the spot itself set apart for the "*sordidiores artes*," and that what was attributed to them proceeded from the marshy nature of the soil. Certain it is, without recurring to this explanation, we cannot reconcile Mr. Bevington's and Mr. Newman's two respectable living witnesses, with the testimony of Ramazzini and Mercurialis, Martial and Juvenal, as quoted by him; and there is sufficient evidence that the *Transtiberina Regio* of Rome, and the *Paduano* (once, 17th century, male *sanus, bestiis quam hominibus aptior*) were proverbially unhealthy from their marshes, and that *Bermondsey* is not: See Ramazzini, *De Morb. Artific. cap. 15.*: and *Annotat. in Lib. Lud. Cornellii, Veneti de Vit. Sobr. Commodis: and de Virg. Vestal. Valetud. tuend Dissertat.*

5. I borrowed the following singular fact from the ingenuous and experienced Ramazzini. "*In hac civitate (Modena), quæ pro suo ambitu satis populosa est, ideoque domos confertas habet ac præaltas, mos est ut tertio quoque anno in singulis domibus cloacæ expurgentur, quæ per vicos discurrunt. Cùm ergo domi meæ id opus fieret, contemplatus unum ex operariis istis in antro illo Charonæo magnâ anxietate ac sollicitudine opus suum peragentem, miseratus tam improbi laboris, ipsum interrogavi, cur tam sollicité laboraret, et non pacatius id ageret, ne ex nimio labore in multam lassitudinem incideret, tunc miser ex antro illo oculos attollens, meque intuitus: nemo, inquit, nisi expertus, imaginari potest, quanti constet, plus quàm quatuor horis in hoc loco morari, idem enim est cæcus fieri. Rursus ab eodem quæsi, num in faucibus ardorem ullum persentiant, difficultatem aliquam respirandi patiantur, capitis dolore tententur, num odor ille nares percellat; nauseam pariat; nihil horum respondit ille, neque pars ulla in hoc opere mulctatur, præter oculos.*" This account was afterwards confirmed by his observing a number of these people reduced to blindness and beggary. "*Oculis tamen solummodo, bellum*

tam atrox indicunt fœtidæ exhalationes istæ, ac illos acutissimis spiculis sic feriunt, ut illis vitam, id est lumen, eripiant." Thus, as certain acrid substances seem exclusively to affect different and distinct parts of the body, as cantharides the bladder, the torpedo the nerves "sic halitus illi ex humanis fœcibus per varios corruptionis gradus trium annorum spatio, talem adsciscant naturam, ut oculos tantùm lacessant, cæteris vero partibus ignoscant." (De Morb. Artific. cap. 13.) This fact is no less important than curious, as it tends to show the inconsiderate conclusions of some eminent writers respecting the influence of the exhalations of privies on the health of men. Now whether the effect of these exhalations is asphyxia at Paris, according to Sauvages (Nos. Meth. i. 820), or amaurosis at Modena, according to Ramazzini, in either case there is ample proof that they cannot be productive of putrid or pestilential fevers.

6. The spontaneous extrication of putrid vapors from the sepulchral vaults of the cathedral church of Dijon, celebrated for having given occasion to the first experimental essays of oxygenants in the decomposition of putrid and contagious effluvia, may seem an exception. But this, in truth, can be considered only as a mephitic acting on those within its influence, in a greater or less degree, according to its concentration, and producing the usual effect of such effluvia, asphyxia. What M. de Morveau says of the appearance of a contagious fever in the neighbourhood, as connected with this mephitic vapor, is extremely vague and indeterminate. By sprinkling with a considerable quantity of the vinegar of the four thieves, "the odor of the putrid effluvia was merely masked for a moment, and soon reappeared with its former activity, *spreading to the neighbourhood, where the symptoms of a contagious fever began to appear.*" When the fumigation had been completed, he does not say that this contagious fever had been stopped or prevented: in fact he says nothing about it: he adverts only to the purification of a mass of air contaminated by these mephitic vapors. (See his Treatise on the means of purifying Infected Air, translated by Dr. Hall, p. 25-29. See also Sauvages,



Nos. Meth. tom. i. 819.) Ramazzini is by no means satisfactory on the subject, indulging in declamation, and exhibiting no proofs that "*post magna prœlia commissa, per insepulta cadavera, seu per antiqua sepulchra incautè aperta, diras pestilentias enatas, quæ ingentem populorum stragem ediderint.*" De Vespillonum Morbis.

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### *A Case of Death, produced by Arsenic.*

By JOHN YELLOLY, M. D. Physician to the London Hospital.

From the Edinburgh Medical and Surgical Journal, for 1809.

I AM induced, by the remarks made in the last number of the Edinburgh Medical and Surgical Journal, on the necessity of attention to individual cases of death by poison, to transmit to the editors the following history and dissection.

On the evening of the 21st of April last, I was requested to see the apprentice of a goldsmith, in Bell alley, Lombard street, who was reported to have taken some arsenic the night before. I went immediately, but, on my arrival, found that he had just expired. He had been attended by Mr. Unwin, apothecary, of Widegate, St. Bishopsgate, and from him, and the individuals of the family in which the young man lived, I obtained the following particulars.

He was about sixteen years of age, and informed Mr. Unwin, that, in consequence of a few words with his master, the day before, he had resolved to destroy himself; and that, at about ten o'clock the same evening, he drank off some gruel, in which he had dissolved, in a saucepan, over the fire, about a pennyweight of the white oxide of arsenic, of which there was generally some kept in the shop, for the purposes of the trade.\*

On examining the saucepan, Mr. Unwin found in it the remains of the gruel, and about twenty-six grains of the white oxide of arsenic, in rough powder, though the lad told him,

\* Arsenic, I am informed, is used in making a solder for inferior descriptions of gold work, called arsenic solder.

that he had only employed about a pennyweight, or twenty-four grains.\*

His master's daughter observed him, in the course of the evening, throw something from a paper into a saucepan of gruel, and simmer it for a little time upon the fire. She and the maid servant saw him drink off the gruel; and the servant, having taken a tea spoonful of it, by his desire, was made very sick and uncomfortable the whole night. With regard to the quantity of arsenic which had been taken, it was supposed, from the diminution which was observed in a small packet of it, bought a short time before, that it must have been about half an ounce. But this is only conjecture. At any rate, the lad must have been much mistaken in the quantity which he imagined he had used, as there was more found in the remains of the gruel than he stated that he had put into it.

Soon after the gruel was taken, thus charged with arsenic, he was attacked with vomiting, and, in a short time subsequent to this, with purging; both of which continued with violence the whole of the night. He drank large quantities of both warm and cold water, but did not complain of any pain in the stomach or bowels, nor did it appear to the family that he suffered any. About four hours after taking the arsenic, he complained of a severe cramp in one of his legs. He was much distressed with a sensation of cold during the night, which made it necessary for him to have a great quantity of bedclothes.

Mr. Unwin saw him, for the first time, at half past ten on the morning after the arsenic was taken; his pulse was then not more than 40, and very languid. His lower extremities were cold, stiff, and without the power of motion; but, by the application of warmth, the stiffness was, in some measure, diminished, and the power of motion, in a small degree, restored. Mr. Unwin kept up the vomiting, by means of emetics and copious diluents; but the purging ceased about twelve

\* As a proof that there was no mistake with regard to the nature of the powder found in the remains of the gruel, it may be proper to mention, that when made into a solution with carbonate of potash, it afforded a lively green precipitate on the addition of a solution of sulphate of copper.



o'clock, and the means employed to bring it on again were without effect.

At half past three, the extremities were cold, and he seemed unable to move them. He complained of great heat in his tongue; was very restless, and his pulse was rather more frequent than in the morning. The vomiting at this time had ceased. At about five, he appeared to be sinking fast; his pulse was as low as 30, and very languid; the upper, as well as the lower extremities, were cold and motionless; the restlessness was increased, and he expressed a great wish that sleep should be procured. He died a little before seven, which was nearly twenty-one hours after taking the arsenic. He retained the complete possession of his faculties till the very last, and never discovered any particular irritability of mind, except once or twice, when he could not get drink at the moment he wished for it. He was, at no time, in any degree convulsed, and showed no appearance of delirium. Mr. Unwin did not remark any tenderness of the abdomen, and never heard him express the existence of pain, either in the stomach or bowels. Indeed, when repeatedly questioned by him upon this point, the young man said that he felt no pain any where.

Mr. Unwin did not particularly attend to the sensibility of the extremities; but it was observed by him, that the changes of position which the young man was continually making, a few hours before his death, were effected principally by the trunk of his body, the extremities appearing inert.

The body was opened on the evening of Sunday, the 23d of April, about forty-nine hours after his death, by Mr. Morris, one of the house pupils of the London hospital, in the presence of Mr. Unwin and myself. It exhibited no particular signs of putridity; there was a slight lividness in the inside of the thighs, but nowhere else. On opening the abdomen, the stomach was found to be very large, but with no external mark of inflammation. The whole course of the small intestines, to within three inches of the caput coli, was very much inflamed, and the duodenum and jejunum were a good deal thickened. In some places, the external surface was of a florid red; but it was, for the most part, of a purple hue, and with occasional

patches of coagulable lymph. That part of the ilium which was free from inflammation (which was, as I have already said, a space of about three inches,) possessed its natural appearance, as did the whole of the cæcum, colon, and rectum. The colon and rectum, through their whole extent, were smaller than ordinary.

The inner membrane of the stomach, particularly at its lesser curvature, and in the neighbourhood of the pylorus, was very much inflamed, and there was in it frequent appearances of extravasated points of blood. In two or three circular spots, of the size of a shilling, the membrane was abraded; and, in one or two places, there was a circular thickening, (as if by an effusion of coagulable lymph,)\* with some extravasated points of blood upon it. The stomach contained a considerable portion of a yellowish white fluid, with some castor oil floating upon it, which had been given in the course of the day of his death. In this fluid were found a grain of shot, a few small bits of carbonaceous matter, and a very small portion of a heavy, white, coarse powder, resembling the white oxide of arsenic.

The inflammation did not extend to the œsophagus. Portions of the small intestines were examined at several places, through their extent. The mucous membrane was every where found in a high state of inflammation, and with occasional appearances of extravasation in it. In the mucous membrane of the large intestines, the inflammation was much less, but it extended, in some degree, to the extremity of the rectum. There was no portion of arsenic or fæces found in any part of the alimentary canal.

The cohesion of the stomach and intestines seemed to be as great as in ordinary circumstances.

The absence of pain is a circumstance in which this case seems to differ from any which I have seen recorded; and I was therefore extremely particular in ascertaining this fact, not

\* Dr. Baillie mentions having seen a thin layer of coagulable lymph, thrown out upon a portion of the inner surface of the stomach, in a case of death by arsenic.—*BAILLIE'S Morbid Anatomy*, p. 138.



only from Mr. Unwin, but from every individual of the family who saw the young man previous to Mr. Unwin's being called in. The accounts were uniform and consistent; and there is the less reason to suppose any misconception upon this point, because the excruciating pain, which generally follows the exhibition of a large quantity of arsenic, could hardly, if it did exist, be concealed by the sufferer, or overlooked by the attendants.

It is rather unfortunate, that the precise quantity of the arsenic which was taken, in this case, is not known. The young man's own account, as above stated, was evidently far from the truth; while the diminution which was observed in the packet, may be considered to afford some grounds for believing that the quantity taken was very large.

To what extent the effects of arsenic are modified according to its quantity, is a circumstance concerning which there is little evidence; for, very few histories of persons poisoned by that substance have been detailed. That the sensorium is affected by its influence, is rendered probable, by the state of the pulse and of the extremities, in the case which I have now communicated. How far this affection of the sensorium is able to account for the absence of pain, can be at present only matter of speculation.

The effect of very long continued vomiting and purging, and of the use of a large quantity of diluents, would be to remove the poison which was swallowed. It may, therefore, be considered as probable, that the agency of such a substance may be sufficient to destroy life, though a very small portion, or none of it, should be retained.

The prominent effect of arsenic, taken into the stomach, is the production of inflammation; but this effect, like inflammation from any other cause, will doubtless continue after the cause producing it has been removed. There is, therefore, some reason to suppose, that the means generally employed in the treatment of such persons as have taken arsenic, whether they are directed to decompose, or to carry off this substance, are deficient, inasmuch as the inflammation actually produced by it is overlooked. The only remedy on which we can de-

pend for the cure of inflammation of the stomach or bowels, is copious bloodletting; and it may, therefore, be a question, how far this, in conjunction with the other means which may be thought necessary, should be early had recourse to, in cases where arsenic has been swallowed. Analogy seems to indicate its employment; but its particular fitness can only be determined by experience.

Finsbury Square, June 5th, 1809.

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*The following abstract of a case of Death from Arsenic is also deemed worthy of insertion. It may be consulted at large in the Transactions of a Society for the improvement of Medical and Chirurgical Knowledge. Vol. ii. p. 63.*

MARY HUNT, on Thursday, the 19th of April, at twelve o'clock at night, took half an ounce of white arsenic, and immediately afterwards drank a quart of wine; about one o'clock she had so much pain in her stomach as to be obliged to call for assistance.

The symptoms were excruciating pain in the stomach, sickness, vomiting, excessive thirst, and a small tremulous pulse; these were followed by pain in the bowels, and several purging stools.

She drank brandy and water, wine and water, and several quarts of plain water to relieve the thirst, and ease the pain. Some hours after taking the arsenic she became easier, expressed a desire to be left alone, being inclined to sleep, and remained several hours in a dozing or comatose state, from which she did not recover, and died about one o'clock on Friday, thirteen hours after taking the arsenic.

Upon inspecting the body after death there were found the following appearances.

In the cavity of the abdomen there was an appearance of the effects of slight inflammation on the peritoneal coat of the small intestines.

The stomach contained a greenish fluid with a curdy substance in it, in all amounting to about twelve ounces. On the



internal surface of the great curvature near the cardia a portion of the villous coat about the size of a crown-piece was partly destroyed, and of a dark red color, with a regularly defined edge, and some of the arsenic adhering to different parts of its surface. The rest of the stomach was in a natural state. This appearance in the stomach was an effect produced by the arsenic.

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*Singular Case of Lameness.*

Communicated in a letter from Dr. OSBORNE, of Cork.

From the Edinburgh Medical and Surgical Journal, for October, 1810.

A. B., aged twenty-three years, was, on the 3d of June, 1808, as well as ever in his life; but the next day, when marching with his regiment in Guernsey, for embarkation, was suddenly attacked with pain in his right side, so violent, that he could not stand; was shortly after brought to England, where the staff-physicians were of opinion that his liver was badly diseased. He was sent to his father in Ireland, where Drs. Pigot and Drew gave it as their opinion that he could not live. On this Dr. O. was sent for, who found him dreadfully reduced; his emaciation and debility excessively great; his tongue, from the manner of articulating, showing strong threatening of paralysis; pulse 120; tongue dry, with a dirty white crust upon it. The liver, on examination, found to be sensibly enlarged, and giving the feel as if pressing a tough dough or clay. His urine, in 24 hours, did not amount to three noggins, very high-colored, and depositing sediment between red and white. Bowels costive; affected only by medicine; appetite quite gone; his *right leg by three inches shorter than the left*; which, upon inspection, was found to be in consequence of the head of the thigh bone having slipped out of the acetabulum; and of this he could give no account, unless it happened when on board ship, whilst changing from hammock to hammock; but that he never had any sense of uneasiness or pain, either then or at any other time. From his emaciated state, fever, diminished urine, impediment of his tongue in articulation, Dr. O. conceived he could not live, and that he might be cut off sud-

denly; but ordered the squil-soda and hydrargyrus pill; that he should drink double soda-water, or plain water; no wine; and that his diet should be light and nourishing. In about six weeks, he came to town to have a consultation with Dr. Longfield, when he had the appearance of hale health in every respect, excepting the dislocation, which much surprised Dr. L.; who was so particular in his examination, that he said "the head of the thigh-bone is pushed directly upwards, and here is the protuberance formed by it distinctly to be felt." As the pills that had been ordered had set his kidneys active, by the discharging of large quantities of urine, they were now dispensed with, and he was put on a course of bark. This he continued for the course of a month, and, at the end of the time, returned to Cork, with both extremities of the same length. He was stripped, and the head of the femur was found to be perfectly in the acetabulum.

This is a most extraordinary case, and can be only accounted for from an excessive degree of relaxation of the rotund and capsular ligaments. For, if the bone had been forced out accidentally, a rupture of the ligaments must have been the consequence, and of course the bone would remain out; but the contrary must have been the case; for as the constitution recovered its natural powers, so the ligaments recovered theirs, and, by gradual contraction, brought about the reduction.

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### *On reducing Dislocations.*

By BRUDFORD WILMER.

From the Medical and Physical Journal, for December 1810.

THE following remarks on dislocation of the limbs have occurred, on reading a very interesting paper on the subject, published in the Medical and Physical Journal in April, 1808. After offering some very sensible strictures on the present mode of reducing luxations, in counteracting, by violent extension, the resistance of the muscles, (as he emphatically terms it) *by main force of surgery*; Mr. Carwardine proposes



to apply a regular and long continued extension, to exhaust their action. This he illustrates by the following case.

A strong muscular man dislocated his shoulder. Attempts to reduce it were ineffectually made in the country where the accident happened; and after an interval of fourteen days, he applied to an hospital which Mr. Carwardine was then attending.

One of the senior surgeons proceeded to the reduction of the joint. After repeated and violent attempts, which proved ineffectual, it was proposed to the man to remain in the hospital till the following day, and in the interim, that he should lose blood, take a cathartic, and keep a relaxing poultice on the joint. His business, however, not allowing of a longer continuance in the hospital, he was about to quit it, when Mr. Carwardine persuaded him to go to his lodgings. Having provided the necessary apparatus, the patient was placed on a stool, the scapula and trunk firmly fixed, and a set of pulleys accurately adjusted and secured to the condyles of the humerus, for the purpose of making extension. The cord was now tightened with a force just sufficient to counteract the gravity of the limb, and prevent its falling to his side. Mr. W—— with one hand held the wrist, to preserve the forearm bent, and with the other held the cord to keep it from relaxing, but without tightening it. This extension was continued for ten minutes, without producing any manifest effect. As the extension gave very little pain, it was slightly increased. In five minutes more the man grew very restless, would not allow that he then suffered pain, but said, *he felt very faint*, begged that Mr. C. would desist, and asked for water which was refused, under the idea, that if he fainted the muscles would be completely relaxed. At this moment he changed countenance, and the deltoid muscle was perceived to be tremulous. The cord was tightened a little: the arm followed it; the cord being now relaxed, the head of the bone slipped into its place, and the reduction was accomplished without any perceptible sound.

Mr. Carwardine attributes the success of this experiment to the long continued extension, having exhausted the action of those muscles connected with the dislocated limb; but from

cases which have occurred in my practice, it seems more probable it was the effect of that disposition to faint, which the man complained of towards the latter end of the experiment. In a state approaching to syncope, the circulation becomes weak, the action of all the voluntary muscles is nearly suspended, and if at that favourable moment, an extension could be made in cases of dislocated bones, I believe it would be generally successful. Surgeons who have seen much of practice must have remarked, when they have been called to patients who have suffered these accidents in a state of intoxication, with how much comparative ease, the reduction has been accomplished. In this state all the powers of the body are in-feeble, and the muscular action, for a time, almost destroyed.\*

Previous to the action of an emetic, the face becomes pale, and faintness is generally induced. At this critical time, if an extension is made, the reduction of luxated bones is accomplished with a facility, that will surprise those who have not seen the experiment. When cases have occurred in my practice, which have resisted the common methods of reduction, I have ordered an antimonial emetic, and making an extension when the patient first complains of faintness or sickness, have always succeeded, and with very little exertion of force.

Mr. Lloyd, aged about forty, a very strong and muscular man, dislocated his shoulder by a fall down a precipice; as it was late at night, he got no assistance till the following day. At seven o'clock in the morning, an eminent surgeon of this city was called to him. The trunk of the body was firmly fixed to a wall, and extension made from the lower part of the humerus, with the forearm bent, without success. Pulleys were then applied, and much force used, but the head of the bone was immovably fixed. Extension was then made in every direction in vain. These experiments were reiterated during the space of an hour, till the patient and the surgeon were both much exhausted. At eight o'clock, I was called into consultation, and proposed, that five grains of tartarized antimony

\* Mr. Chesher has published a case, in which the reduction of a luxated *Os Humeri* was facilitated by the exhibition of emetic tartar. *Med. Journal*, vol. 8, p. 189.



should be given, every thing being prepared for extension; in five minutes the patient said he felt ill, in three more that he thought he should be sick. By a moderate degree of force applied to the lower part of the bone, in the same direction as that first applied, in a few seconds the reduction was accomplished with very great ease.

On the fourth of September last, a stout muscular young man of this city was thrown out of an open carriage and dislocated his arm. The next day I was desired to assist his surgeon, Mr. Kann, in the reduction. He had a very bad night, without sleep, in constant pain, was feverish, and the parts over the joint, tense and swelled.

A very forcible extension was made in the usual way, but the head of the bone was firmly fixt under the pectoral muscle. The patient was in a state, in every respect, the most improper, for the application of much force; I, therefore, proposed to Mr. Kann, to give him five grains of tartarized antimony. In seven or eight minutes he complained of being faint. I now remarked to Mr. Kann, the alteration which had taken place in the state of the parts about the joint. The muscles, before hard and tense, had become soft and flabby. Extension being then made in the same direction and manner as before, the reduction was immediately accomplished.

It was the opinion of Dr. Hunter, that in complete luxations of the os humeri, the capsular ligament was always ruptured, and that the nature of the laceration would sometimes render the reduction impracticable. And Mr. Thompson\* has supposed, that the head of the bone might be tightly embraced by the tendons of the subscapularis and teres minor muscles, passing obliquely over it, and thus form an impediment to reduction. I believe it is now generally admitted, when these difficulties occur, that they are occasioned by the muscular power counteracting, with effect the extending force applied to them. Mr. Pott, aware of this circumstance, has given precise directions (by the position of the limb) to place the muscles, sur-

\* Medical Observations and Inquiries, vol. 2, p. 355. Potts's Works, fol. edit. p. 693.

rounding and connected with luxated bone, in that state where they are likely to give the least resistance. But it will be found impossible to place a limb in any position, in which some of the muscles will not be more or less elongated. If the flexor muscles are relaxed, the extensors are, in the same proportion, stretched. Hence, it appears to be a great desideratum to affect the system in such a manner that *all the muscles* connected with the luxated bone may for a short time be deprived of their powers of making resistance.

In cases where great force and violent extension have produced no effects upon the dislocation, it has been the practice of surgeons to reduce the strength of the patient by large bleeding, baths, cathartics, and to relax the parts by fomentations, poultices, &c. That these sometimes fail is too well known to be insisted upon. The exhibition of an antimonial emetic in a few minutes reduces the muscles into a state of inaction; the other plan will not have the same effect in as many days. From so much evacuation constitutional effects may arise. From the former plan the patient's general health may be improved, and his recovery will be very speedy. He should go to his bed after the operation of the emetic, when a perspiration ensues, which will remove that feverish action of the system, the general accompaniment of accidents of this nature. The muscles also at the time of the reduction are so much relaxed that no pain attends the operation.

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*Extract of a letter to N. Bruce, Esq. Surgeon to the Forces, concerning the Endemial Fever of Sicily. Originally communicated in September, 1809, to Dr. Franklin, Inspector of Hospitals.*

By ALEXANDER BOYLE, Surgeon, 62d Regiment of Foot.

As I had observed, some time before the return of the troops from Ischia, a disposition in the fever, prevailing in Melazzo, to assume the form of synocha; and aware of the ravages *fever* had made among the corps quartered in that district, for some seasons previously to my visiting that part of Sicily, I resolved to watch its progress.



In the first cases which offered themselves to my notice and which occurred about the beginning of July last, there appeared little which could, in a particular manner, demand my attention, beyond what is generally observed in those fevers to which we usually give the name of synocha.

At this period (from which we may date the commencement of the disease this season), it was ushered in with a cold stage; there was great languor and lassitude; hot dry skin; frequent and full pulse, and great thirst. The tongue was white and moist; the countenance flushed; and the eyes more or less diffused with blood. Head-ache, giddiness, and lassitude were, however, the symptoms of which they chiefly complained.

Until the middle of July, however, cases of fever were very rare; and such as did occur were easily overcome by cold affusion, a free use of saline purgatives, and plentiful draughts of iced lemonade. Bloodletting, at this period, was not necessary. In two cases, which occurred in children, the disease terminated by hemorrhagy from the nose; and in both, it was followed by a pustular eruption about the head and face. In a healthy girl of seventeen years of age, it followed the sudden interruption of the menstrual discharge, and ceased soon after that reappeared.

Though I kept no regular meteorological tables, I remarked that the weather, from about the 1st of July, became extremely oppressive, and that no rain had fallen for several weeks; the thermometer usually ranging from 84° to 88°, and a Sirocco wind generally prevailing. But the facility with which the disease was in these cases overcome, as yet led me to expect nothing extremely alarming.

On the 15th, however, two men came from the telegraph post at Archi, near Melazzo, which had always been deemed an unhealthy spot. Their cases, of course, became extremely interesting. From the great mortality at this place, it was never visited without apprehension. It became, therefore, a matter of importance to ascertain how far such apprehension was well founded. I went there, accompanied by Dr. Calvert, physician to the forces; and we were aware that the situation was reputed unhealthy, from exposure to what, in Sicily, is

generally called *mal' aria*. If by this is understood nothing more than *marsh miasma*, it may be said, that, though in autumn it may become the parent of many intermittents, at the time of our visit, there appeared no probable source from which it could be produced; and except one small inconsiderable stream, there was not a drop of water within two or three miles of the place. We ascertained, however, that the daily duty of the three men at that telegraph, exposed them alternately to the action of the solar heat, as long as it was possible to communicate with the adjoining posts. This discovery was at least sufficient to satisfy us, as to the object of our inquiry; and, without the assistance of other agents it will be allowed to be adequate to the production of the appearances discovered on dissection, in two cases I am about to relate.

The first whom I saw, observing the situation of his two companions, hastened to Melazzo. He rode about five miles. His pulse was weak and trembling, and about 110 in the minute. The muscles of his face were relaxed, and his countenance was pallid. His eyes were not red and suffused with blood, but had a peculiar lustre; and the pupils were much dilated. There was an unusual secretion of tears, and a discharge of mucus from his nostrils. His skin was cool, and covered with sweat. His tongue was white; and his bowels had been costive for several days. He complained of giddiness, headache, and lassitude. He was able to give some account of the two men he had left behind, but his recollection often failed him. Though he could stand erect and walk, his eyes appeared to deceive him, as to the relative situation of objects, and he stretched out his hands to direct himself. He had been unwell eight days; and his complaints began with languor, lassitude, giddiness, and headache, which had always continued, but had not before prevented him from attending to his usual duty. His general appearance approached nearly to that of a person affected with slight concussion of the brain, or in a bad state of typhus. By this, unfortunately, I was deceived; and, allowing myself to be guided by the state of the pulse, I did not bleed him. I ordered a large blister to be ap-



plied to the nape of his neck; he took eight grains of calomel, and a purging clyster was immediately given. Next morning (the 16th) he could scarcely give any account of his feelings; his pulse had become somewhat fuller, but did not continue regularly so during the whole day. I now observed some slight twitchings of the muscles of the face. He had several evacuations by stool, during the night; and this morning he vomited much bilious matter. The temperature of his body was now greater than natural, and his skin dry. I ordered his body to be sponged over with cold water; his head to be shaved; and snow and iced water to be applied to it. Though he was in a state of coma, this application produced such uneasiness, that it could only be continued for a few minutes. Towards the afternoon he became much worse; and the pupils, which continued dilated from the beginning, were now quite immovable. His breathing became very laborious; his pulse slow and irregular; he foamed at the mouth; and was occasionally convulsed.

Though I was convinced the favourable opportunity was past, I resolved to take some blood from him; and I accordingly opened the temporal artery. The effect surprised me; for, while the blood was flowing, his breathing became perfectly easy, which encouraged me to take a pretty large quantity. In the evening, however, his breathing again became laborious, the convulsions very frequent, and early next morning he died.

On opening the skull, nothing uncommon was discovered on the external surface of the dura mater. Its sinuses, however, were gorged with blood, and, the small veins, leading to the longitudinal one, much distended. Between the dura and pia mater, there was a small quantity of serum effused; and the surface of both, in the neighbourhood of the longitudinal sinus, was covered with that sort of matter which is observed on the peritonæum and other membranous parts which have suffered from inflammation. Adhesions had also taken place; and in two or three places small abscesses were formed. In the ventricles, and within the substance of the brain, nothing uncommon was found.

The treatment of the other patient fell to Dr. Calvert, and

was nearly the same as in the above, with the exception that he was not bled. He was admitted into hospital on the night of the 15th, and died on the evening of the 16th, having been unwell about six days. His death was more sudden than in the other, and was not preceded by the same violent symptoms. The pupils always showed more contractility.

On dissection, fewer adhesions were found between the membranes of the brain; but the same sort of matter was poured out on their surface; and this also more particularly contiguous to the longitudinal sinus, which was gorged with blood. The substance of the brain appeared more vascular, and was much redder than usual. In one of the ventricles there appeared a few red globules, diffused through the serum; which, in neither, exceeded the usual quantity.

Though we had been unsuccessful in the treatment of these cases, it affords me much satisfaction to have been enabled, by the appearances on dissection, to form a more correct idea of the nature of the disease than that generally entertained on the subject. In these cases, I suppose, from the slow and insidious progress of the disease, that the inflammatory action, during the first days, had not been very violent, and might, perhaps, have been what Dr. Paterson calls a *phrenicula*, or what nosologists distinguish by the name of *cephalitis*; for, during a whole week, they continued at their usual duties, and never complained until the disease had assumed that fatal and deceitful form peculiar to the *second* stage.

I can easily conceive that these symptoms have led many into error; and that, whatever may have been the treatment at the beginning, from mistaking the symptoms of effusion for those of the debility of typhus, barks and wine were immediately had recourse to, and thus hastened the fatal termination.

On the 31st of July the 62d regiment disembarked at Melazzo; and in a few days *this fever* made its appearance among the men. The above cases may be sufficient to show the appearance of the disease throughout the whole of the *second* stage; and, as out of 240 patients admitted into hospital, affected with it in every degree of violence, not one case terminated fatally, I shall endeavour to point out the symptoms



peculiar to the *first* stage of the disease, as they were observed in that number.

It did not appear to attack the robust and plethoric more frequently than the weak and delicate; though, in the former, its symptoms were always more violent. In the majority of cases it was ushered in with a cold stage; while some complained of giddiness and languor for several days previously to the accession of any regular paroxysm, which, however, was always a certain occurrence. When thus suddenly attacked, they complained of violent headache and giddiness, with throbbing pain in the temples, attended also with languor and lassitude. The pulse, in the early part of the disease, was full and strong, and often exceeded 100 strokes in the minute. The countenance was most commonly florid; but when giddiness had for a long time preceded the violence of the attack, it was pale and relaxed. The eyes, which were but seldom suffused with red, had a remarkable lustre, and were extremely sensible to light, which, as well as the least noise, gave great uneasiness. The bowels were always costive. The tongue was generally moist, and invariably covered with a white or yellowish mucus, and red about the edges. Though they generally complained of thirst on the first days of their illness, it never formed an urgent symptom; and the sensation entirely abated as the disease advanced. The skin, at the beginning, was hot and dry, the temperature often  $102^{\circ}$ ; and, when the disease was about to terminate favourably, profuse sweating often occurred. Though there was in every case great drowsiness, approaching to stupor, they never enjoyed sleep. I never observed, even in the most severe cases, phrenitic delirium. They, on the contrary, lay motionless in bed and appeared uneasy when disturbed, or roused from their apparent slumbers. Nausea was always present; and in three cases it was attended with obstinate bilious vomiting. During the course of the disease, I have sometimes met with pulmonic symptoms.

It appears to me, that the cases which occurred, at the first appearance of the disease, were, *in general*, more violent than they afterwards proved to be, towards its decline, or when it was becoming to be less frequent. This took place about the

first of September; the weather had become cooler, the thermometer having fallen to 78°. There were also some showers of rain, with thunder, about this time.

Among the vulgar, both in Sicily and Calabria, most of the diseases peculiar to the climate, particularly those of a malignant nature, are attributed to what they call *mal' aria*. This language has even been adopted by some professional men; and I dare to say, the investigation of the nature and origin of diseases has thereby been not a little retarded. If it is meant to imply nothing more than *marsh miasma*, there can be no necessity for changing the phrase, particularly, as that substituted is by no means more philosophical or scientific; and as such change can only throw obscurity on a term, the import of which is already well understood. To this cause, then, the fevers prevailing in Melazzo and its neighbourhood, during the hot months of summer, had for a long time, been attributed; and it was solely from the information gained by dissection, that we have been enabled to trace, with much certainty, those fevers to the powerful action of the solar heat, occasionally aided by intemperance, violent exercise, and the like.

While the excessive heat prevailed, the character of the disease continued invariably the same; but, as I have already observed, about the 1st September, it not only became less violent in its symptoms, but also occurred less frequently. From the disembarkation of the 62d regiment, on the 31st July, until the end of August, it was common to see sixteen or twenty soldiers, affected with it, brought to the regimental hospital, in the course of one day; but, between the 1st and 10th September, only fourteen cases appeared; and, as the weather became cooler, with frequent showers and strong winds, it entirely gave place to diseases of a different nature.

I have only met with three cases in which it was followed by intermittent fever; and I have since met with one case, in which during the state of convalescence, the patient was seized with remittent fever, obviously observing the tertian period, and accompanied with severe diarrhœa. But it must be observed, that these were the only cases of the kind which did



occur; that they appeared at the close of the season, and were sufficiently distinct in their nature.

Soon after the disease appeared among the men, the necessary means were taken to counteract and avoid its causes; and in order to afford more room in the barrack, one company was sent on board a vessel anchored in the harbour; and it is worthy of remark, that though it continued embarked nearly during the whole time the regiment was quartered in Melazzo, a period of eleven weeks, it afforded only one solitary case. Even among the companies ashore, there was a striking difference with respect to the proportion of men attacked. In one of the quarters, the men slept in the walk surrounding the area of a convent, exposed to the open air, and during the day, they always availed themselves of the protection it afforded from the excessive heat of the sun. In this company only ten men were attacked, while in some other companies, which did not enjoy these advantages, the number exceeded thirty. These circumstances, which were so useful in preventing this company from being seized with the prevailing disease, exposed it, however, to those of a different nature. Diarrhea and rheumatism were common in it; and it furnished more cases of ophthalmia than all the other companies put together.

Among the diseases affecting the brain, or its membranes, we must expect to find a strong analogy, and many symptoms in common. Those which the disease in question most resembles are cephalitis, phrenitis, and hydrocephalus internus.

I have already said, that (in upwards of 240 patients all soldiers,) I did not observe a single instance of wild and furious delirium. Even when the pulse was full and quick, giddiness and a tendency to deep sleep, were the symptoms which first caught the attention; they were the first complained of by the patient, and were always the most lasting. So far the disease differed from phrenitis, strictly so called. It must also be observed that, in the only two cases in which I had an opportunity of examining the appearances after death, the inflammation had not only affected the dura and pia mater, but had greatly injured the organization of the brain itself. There was congestion in the sinuses, and effusion of serum and coa-

gurable lymph. How far this complication of morbid appearances may associate the disease with cephalitis\*, or account for the peculiar modification of its symptoms, I shall not take upon me to determine; neither am I very anxious to ascertain in what part of the organ the inflammation commenced, as such discovery could, in no degree, influence the mode of treatment.

It would, indeed be an endless, and not less an unprofitable task, to attempt to mark every accidental modification of disease by an appropriate name, or to assign for it a place in nosological nomenclature. "Imo etiam ubi distributio in species reperitur, id fit plerumque, ut hypothesi alicui, quæ veris phænomenis substruitur, suus reservatur honos; ac proinde ejusmodi discriminatio non tam ad morbi, quàm ad authoris ingenium, philosophandique theoriæ accommodata est. Quantum medicinæ obfuerit *ætiologia* hæc in parte defectus, ostendunt multorum morborum exempla; quorum nempe curationes hodie non desideraremus, si scriptores, dum experimenta et observationes medicas benevolo saltem animo communicarint, immo scilicet speciei morbus pro alio speciei diversæ substitutus non fefellisset, atque hinc etiam arbitror profectum esse, quod materia medica in sylvam tam immensam sed fructu perexiguo excreverit†."

With respect to hydrocephalus internus the analogy is not so great. It is produced by different causes, and is much slower in its progress. It is peculiarly the disease of early years; often appears hereditary; and is more certainly fatal. Like it, however, it is also marked by two distinct stages, as more particularly noticed in the cases which came from Archi. In hydrocephalus internus, the brain may suffer much from the pressure of accumulated serum before it produce death; in this disease, the quantity of effused serum was inconsiderable, while the destruction of parts by inflammation was very extensive.

Indeed, when an organ so important as the brain, on the healthy state of which, not only sensation, but the due perform-

\* Though this term may be somewhat exceptionable, I have adopted it as it was necessary to make some distinction between *simple fever* and this disease; and as the former term conveyed very vague and erroneous ideas of its nature.

† Sydenhami Opera.



ance of every function depends, becomes the seat of disease, we must expect to find great derangement of the whole animal economy, the symptoms of the local affection not appearing merely as such, but as indicating general disorder of the whole system.

On this account it happens, that between the simple synocha and phrenitis, we can, with no degree of precision, draw the line of distinction\*. To the same cause also it is, perhaps, owing, that in so many diseases described as simple fever, we find the affection of the head forming so conspicuous a feature; in many of which, I am convinced, this local affection is primary, and essential†.

Of this kind, I suspect, are many of the fevers peculiar to warm climates; in the production of which, the intense heat of the sun is allowed to have so considerable a share. Though such fevers belong almost exclusively to the warmer regions, they are also occasionally met with during the summer in more temperate climates. Their progress is, for the most part, quick and rapid, and they cease entirely, or at least are greatly modified, on the approach of autumn.

As I, therefore, have no doubt that the chief cause of the disease which prevailed in Melazzo last summer, and which has hitherto proved the most fatal among the troops in Sicily, was the intense heat of the sun, I have always considered the fever attending it as merely symptomatic of the inflammation within the skull. The manner in which the general febrile symptoms follow, with proportional severity, the affection denoting topical lesion of the organ concerned, is distinct and obvious; and the means had recourse to, and which prove most useful when directed to the relief of the local affection, procure also the most permanent and sudden cessation of the general febrile excitement. I have, therefore no hesitation, guided by the proofs afforded by dissection, in classing this disease with the phlegmasiæ.

\* Wilson, Vol. III.

† May not the fever which lately prevailed at Gibraltar have been of this nature?

Other fevers, however, peculiar to these climates, and in which an affection of the head also forms a conspicuous symptom, undoubtedly belong to the tribe of intermittents; and widely differ from the disease in question. In the writings of Torti, they are particularly described under the title of *Febres perniciosæ comitatæ*; and in book iv. chapter 3. of his *Therapeutice specialis*, we find a collection of cases in which it took place. The fever, which this symptom attended, belonged to the type of tertian remittent, and it was never observed to occur until the *fifth or seventh day, and often at a more advanced period*. It therefore obviously formed a complicated disease, produced by a pure phlegmasia affecting that particular organ, supervening at some uncertain period of the original fever. Bloodletting was often had recourse to in order to relieve the immediate urgency of this symptom; but it was soon followed by doses of bark, by which the cure was finally effected.

In the disease, however, which prevailed in Melazzo, the cause and effect were simultaneous; for, whether the patient suddenly fell motionless on the ground, or complained only of headache and giddiness, these symptoms, whatever interval might take place, I may say invariably preceded the formation of the febrile paroxysm.

In as much as relates to the *period*, at which the affection of the head appeared, it approached more closely to the *causus*, or *febris ardens*. "*Erat enim constitutio februm ardentium in hoc modo. Ab initio soporosi, anxii, horridi; febris acuta, non valde siticulosi, non deliri. De naribus parum stillavit. Exacerbationes plerisque diebus paribus,*" &c.\* These also were most commonly met with during the summer season; they affected chiefly young robust people who used much exercise, or happened to be travelling, and did not sufficiently guard against the influence of the sun; or such as exceeded in the use of hot stimulating diet; and, in certain habits of body, they were often complicated with cephalitis and phrenitis.†

The cases here quoted are, however, referred to the "*con-*

\* Hippocrat. 1 sec. 3 lib. 3 epid.

† Burserii Opera, vol. ii.



*tinua remittens.*" In the summer season they showed a stronger disposition to preserve the nature of ardent fever than towards autumn, particularly if intermittents prevailed, or after the inflammatory diathesis, with which they were at first complicated, had been subdued.

It would almost be endless to describe every variety of fever of this kind mentioned by authors. I shall, therefore, advert only to another; which, I am inclined to think, was a disease of the same nature as that observed in Melazzo during this and former years. It is to Lucian we are indebted for this singular record. The disease appeared at Abdera, during the middle of summer; it was evidently produced by the intense heat of the sun; its attack was sudden, and not one in the whole city escaped it. The delirium which attended this disease was not of the furious and ungovernable kind; and it terminated about the seventh day, with profuse sweating, or hemorrhagy from the nose. The circumstance which, in the relation, most strikes our notice, is the pertinacious manner in which the mind pursued the same train of ideas which had grown familiar by habit, or had been made powerful by strong momentary impressions. It is said to have first broken out among the citizens at the theatre, where they had assembled at noon, while they attended, with admiration, to the performance of one of the plays of Euripides.

The story, however, is perhaps too well known to require to be related here.

Taught, as I was, by the fatal issue of the cases which came from the telegraph, I could not long hesitate as to the method to be pursued in the treatment of this disease. Some slight cases, as at first, were cured by cold affusion and purging; but to these remedies alone I very seldom trusted. Copious blood-letting was what I chiefly depended on. In the use of this remedy, I was never entirely directed by the state of the pulse, particularly after some days had elapsed, the headache and giddiness appearing more certain guides; and I often found, that, in such cases, these symptoms were relieved even while the blood flowed, the pulse gradually becoming fuller, and more regular.

It is perhaps difficult to determine, whether bleeding from the temporal artery ought *always* to be preferred to venesection at the arm. Both, indeed, will afford immediate relief for the headache and giddiness; but I think that obtained by bleeding from the temporal artery is, *in general, cæteris paribus*, more permanent.\* Small bleedings, however, from the temporal artery, are peculiarly applicable to protracted cases, where slight headache and giddiness continue after the removal of the general febrile symptoms: and in which such bleedings at the arm, as could produce any relief, would be inadmissible.

As to the quantity to be taken, I have always regulated it by the relief obtained. In a young man, whose pulse was by no means full or hard, but where there was much giddiness and tendency to coma, I took away nearly forty ounces of blood from the temporal artery at one bleeding. In two days he was able to walk about, and quite free from giddiness. Indeed, it appears to me, that profuse bleeding, in this disease, does not leave such permanent debility as in the other phlegmasiæ.

Blistering the head I find another powerful remedy, which ought never to be neglected; and in the circumstance of long continued giddiness just mentioned, it is of the utmost service. It ought to be frequently repeated.

There is always a great tendency to costiveness in this disease, which ought to be removed. I have therefore made it a rule, observing the relief it produces, to keep up a gentle diarrhœa as long as the symptoms continue at all violent.

Under the head of cold affusion, may also be comprehended cold drink, and cold applications to the shaved head. They are

\* I have met with some Sicilian physicians, well acquainted with the nature of this disease. Their method of cure consists chiefly in the application of leeches to the temples, and the use of cold acid drinks. Opening the temporal artery is preferable, as it combines the effects of general and local bloodletting. In the army, bloodletting was first practised in the summer of 1807, in one of the regiments of footguards, with much success; but it required the proofs afforded by dissection, to render that practice general; which did not happen until the year 1809. "With respect to opening the temporal artery, it may be here added, that this mode of bloodletting was practised by Dr. Irvine, in the summer of 1808, with much success.



powerful auxiliaries in lessening the violence of excitement, and never fail to be extremely grateful to the feelings of the patient. I have applied pounded ice to the head; but it produces too much pain to be long endured, and, on that account, cold water may be preferable.

I have not been in the habit of using these remedies in daily succession; but, as it were, all at the same time; as I am convinced that a speedy recovery depends entirely on attacking the disease vigorously in the beginning. Indeed, when this circumstance was duly attended to, it never proved tedious; and the patient was almost always able to get out of bed on the fourth or fifth day.

As the violence of the disease abates, the pulse becomes slower, full, soft, and equal; and I have seen a great number of cases, in which its slowness was very remarkable during the state of convalescence, not exceeding forty strokes in a minute. The tongue also becomes clean, and this is a sure criterion of recovery. Giddiness is always the most lasting symptom; and is pretty steadily accompanied with dilated pupils, and both symptoms generally disappear together.

I have only met with four cases of relapse. In one of them, which was very tedious from the beginning, and in which dull pain in the occiput and severe giddiness were constant symptoms, I gave digitalis and calomel.\* In one patient, who had been much addicted to the use of ardent spirits, and had suffered from the repeated use of mercury, after the general febrile symptoms had been removed, there appeared, for some time, a tendency to chronic mania, which, however, disappeared under the use of nauseating doses of antimonium tartarizatum. In the three cases which, at the close of the season, terminated in intermittent fever, I gave bark; and to that remedy the fever readily yielded.

\* This patient got well soon afterwards, but I do not attribute his recovering to this mode of treatment.

*On the Connexion that subsists in certain cases between Amenorrhœa and Phthisis Pulmonalis, as cause and effect.*

By WILLIAM SHEARMAN, M. D.

From the Edinburgh Medical and Surgical Journal, for January 1810.

DR. CULLEN has asserted, in his description of phthisis pulmonalis,\* that "in the female sex, as the disease advances, and sometimes *early in its progress*, the menses cease to flow; and this circumstance is to be considered as commonly the *effect*, although the sex themselves are ready to believe it the *sole cause* of the disease." There is some ambiguity in this expression; for if the suppression occur as the *disease advances*, how can it possibly be mistaken for the *cause* of the symptoms that *previously* appeared, even by the sex themselves? I apprehend it could not be unknown to Dr. Cullen, that in many instances of pulmonary consumption, a suppression of the menses does certainly *precede* every appearance whatever of pulmonary affection; and it is an interesting subject of inquiry, whether the pulmonary disease is not occasioned by the suppression? and it may be useful to ascertain whether, in certain cases, amenorrhœa does not prove a cause of phthisis in the predisposed? or rather, whether, in some cases of amenorrhœa, an affection of the lungs, resembling phthisis, and sometimes terminating in it, is not one, among several other effects produced by, and dependent upon, the suppression of the menses; to which suppression, therefore, we are to direct our attention, as forming the chief disease, upon the removal of which all the other symptoms will vanish, provided that can be effected before the lungs shall have sustained such organic lesion as to render them incapable of continuing duly to perform their proper functions, so essential to life and health? The progress and connexion of the various symptoms will, perhaps, be best illustrated by a case which occurred to me not long since. A young woman, aged 20 years, of a very delicate habit, but in pretty good health, had experienced about five months ago, an ob-

\* See First Lines of the Practice of Physic.



struction of the menses in consequence of cold; from this time her health gradually declined, she became languid and weak, had frequent pains in her side, and occasionally a short dry cough. Her appetite was impaired, and she became rather emaciated. In the month of July I first saw her; at this time she complained of pain in the left side, occasional fits of coughing, which recurred at uncertain intervals, but generally with considerable violence night and morning. She had scarcely any expectoration, little thirst, pulse 120, appetite impaired, bowels regular, countenance and lips very pale; there had been no return of the menses since their first suppression. A blister was immediately applied to the pained part, and directed to be kept open; five grains of rubigo ferri, combined with myrrh and castor, to be given three times a-day, with infusion of chamomile flowers, and the feet to be immersed in warm water every night. The pain was removed by the blister, and it was healed up. The medicines were continued about six weeks; during this time the pain occasionally recurred, which was always soon removed by a blister. The cough was sometimes considerably better, sometimes aggravated, with trifling expectoration; her pulse was diminished to 90; her appetite improved, and her countenance looked better. I now discontinued my attendance, and have since learned, that the only remedies she afterwards used were expectorants and blisters, occasionally, when there was cough or pain, and she *entirely omitted* all other medicines. She continued without much alteration, excepting that she gradually grew weaker, and within the last fortnight was almost confined to bed, without, however, the cough or expectoration being greater than before. On the Saturday morning previous to her death, she was seized with violent excruciating pain in the abdomen, particularly about the umbilicus, which continued, with more or less violence, for several hours; it then left her, and she seemed much in the same state as before, until the Thursday morning, October 13th, when, soon after having got out of and into bed, without assistance, she suddenly expired.

On opening the thorax, there was found a very general adhesion of the pleura pulmonum to the pleura costalis, yet not

so firmly but they might be easily separated by the fingers; there was a small quantity of fluid in the cavity, not exceeding three or four ounces; the lungs were very dense, and of a dark color, resembling the liver in appearance; portions of them, when put into water, fell to the bottom. Being cut into, some of the bronchial cells contained a purulent matter, but in small quantity, and here and there was a small excavation the size of a pea; a few small tubercles also were found, not inflamed. The external surface of all the small intestines was very red, and seemed to be studded with small red points, resembling the villous coat of the same intestines when injected. There was no appearance of gangrene, neither was there any adhesion between the different convolutions of them, nor between any of them and the peritonæum; the omentum also was extremely vascular; the mesenteric glands were enlarged and hardened in several places; the pancreas, spleen, and other viscera, were of their natural appearance.

That the lungs were injured in a great degree, was evident from the tubercles found in them, and from the pus contained in the bronchial cells, but still the ulceration had not arisen to such a height as to be the immediate cause of death. More extensive affections than the present one are perhaps frequently recovered from, and tubercles may remain for many years with impunity, if no exciting cause of inflammation arise. Although, likewise, there were marks of inflammation upon the intestines, yet this inflammation did not immediately destroy the patient; for the pain had left her some days previous to her death, indicating a cessation of the inflammation; which, as it was not followed by gangrene, cannot be considered as the cause of death. The enlarged state of the mesenteric glands affords a presumption, that the flow of chyle into the bloodvessels might have been interrupted, but certainly there was not sufficient appearance of emaciation in the dead body, to enable us to attribute her death to this circumstance. We must look for the cause of death, not in the affection or destruction of any one particular organ, but in that state of general debility, consequent upon the interruption of a very important function in the female economy; during which state of debility, parts of



the body peculiarly predisposed are very apt to take on disease. In the present patient, there appeared to exist a scrofulous diathesis, wherein the lungs and mesenteric glands are always readily susceptible of disease, from the slightest causes, and always the more readily the greater the state of weakness in the system, however induced. Every kind of weakness is accompanied with irregularity in the action of the living power; hence spasmodic contractions of various parts. This disposition to preternatural contraction is particularly observable to take place in the bloodvessels, in all cases of amenorrhœa, forming the principal symptom in what has been called chlorosis, differing from hectic fever, chiefly, in depending upon a morbid action of the vessels themselves, independent of the absorption of extraneous acrimony. This state of the vessels is *perhaps* at first produced by plethora, arising from a retention of a quantity of blood, which excites the larger arteries to stronger action, while yet the resistance in the extreme vessels in the uterus is not overcome by its increased action, long continued, exhausts the living power, and produces weakness. During this state of general weakness, and morbid action of the bloodvessels, partial inflammations are very apt to arise. No wonder, then, that the lungs, when, from constitutional predisposition, they are so susceptible of disease, should be one of the first parts to take on inflammation. If by suitable remedies the lungs are relieved, (the disposition to general inflammation still continuing from its original cause, the suppression not being removed) some other part is very readily affected; hence in the present case arose the inflamed state of the intestines, during which all the thoracic symptoms disappeared. That this irregularity of action in the vessels, sometimes exciting symptoms of inflammation in one part, sometimes in another, entirely depends upon the suppression of the menses is evident, from its generally ceasing upon the return of that evacuation, and from the perfect restoration of health immediately following it. No occurrence is more common than the attack of cough, pain in the side, and difficulty of breathing in females, soon *after* the obstruction of the menses, and upon their recurrence all these symptoms going off. If this view of the case is founded in fact,

does it not follow, that in pulmonary complaints, arising from suppression of the menses, much mischief may be done, by employing, to any extent, the strict antiphlogistic treatment, to remove these secondary symptoms, since it evidently tends to increase that debility which originally produced them. There can be no doubt, but that when the affection of the lungs has proceeded to a certain extent, it will go on independently of its cause, and is no longer to be removed by the restoration of the suppressed evacuation, but requires the same treatment as if it arose from any other cause whatever. It is in the earlier stages of the complaint, where it is consequent upon the suppressed discharge, that I would direct my attention to the suppression, as the chief disease, and merely palliate the troublesome pulmonary symptoms; and experience has convinced me how easily this may be effected by the application of blisters, repeated according to circumstances, while we are endeavouring to reproduce the suppressed evacuation by appropriate remedies, and to restore strength to the system by the use of tonic medicines and nourishing diet. There is a natural tendency in this *chronic general inflammation* to wear out itself, but as this requires many months, and as, during that time, much mischief may arise in various parts, it is always proper to endeavour to remove it, which is most effectually done by restoring the natural discharge from the uterus. The almost constant failure of success we meet with in our treatment of incipient phthisis by evacuations and debilitating medicines, will justify us in recurring to an opposite mode, founded upon the opinion of its being in many instances a symptomatic affection only. Dr. Beddoes observes, "In some instances, when the fox-glove has removed the hectic fever, and greatly reduced the *expectoration and cough*, the decline shall become almost imperceptible, the patient *frequently appearing chlorotic*, but being really phthisical, as the *event* most commonly, and *sometimes* dissection, have evinced." This serves to confirm the opinion, that the pulmonary symptoms are not in these cases the original idiopathic disease. If it be objected, that this doctrine can only be applicable to incipient phthisis in *females*, I would appeal to the experience of practitioners to determine, whether,



in the female sex generally, the approach of phthisis is not more insidious, and its progress more slow than in men, where we usually find the attack more marked, and immediately consequent upon some obvious occasional cause, the symptoms sooner becoming more violent, and its whole progress more rapid. That this difference in the appearance of the disease arises chiefly from its frequently being rather symptomatic than idiopathic in females, will be apparent, if we find, that when phthisis arises in them from some obvious occasional cause, and where there has *never occurred suppression of the menses*, its attack and progress are more marked and rapid, as in the male. That it is so, I think experience will justify me in asserting; and impressed with this opinion, I cannot but recommend that we should keep in view the probable dependence of the pulmonary symptoms upon the interrupted functions of the uterus, and direct our treatment accordingly,\* at least, until we are convinced upon trial, that the opinion itself is fallacious, and the proposed treatment inefficacious or prejudicial.

London, 21st July, 1809.

\* The frequent success of Griffith's myrrh mixture, so much used formerly, may perhaps depend upon its emmenagogue powers; it certainly was serviceable in a greater proportion of females than males labouring under pulmonary complaints.

## SELECTED REVIEWS.

*The MERCURIAL DISEASE. An Inquiry into the history and nature of the disease produced in the human constitution by the Use of Mercury, with observations on its connexions with the Lues Venerea.*

By ANDREW MATHIAS, Surgeon Extraordinary to the Queen, &c. &c.

From the London Monthly Review, for February, 1811.

THE object of this inquiry is highly interesting, as the development of a series of curious physiological facts, and still more as likely to have a material influence on medical practice. Mr. Mathias attempts to prove that, when mercury has been received into the constitution in too large a quantity, or under unfavourable circumstances, it has a tendency to create a specific and formidable disease; and that, while this process is going forwards, all the salutary effects of the mercury are suspended, or counteracted. It would appear also that this is by no means a rare occurrence, but one which is every day taking place under our inspection, although we have unfortunately ascribed the effects which are produced to a different and frequently to an opposite cause. In the introduction to his discussion the author gives a brief analysis of the plan which he proposes to pursue, and of the positions which it is his object to establish. After having shown that the disease exists, and that it is caused by mercury alone, he proceeds to state that it possesses a uniform and specific character, and that it is distinct from other diseases; concluding from this circumstance that it must have an important effect on our method of treating syphilis.

According to this plan, the first section is intitled a 'general history of the mercurial disease,' and consists of an ample and perspicuous account of its symptoms, both local and general, of the manner in which it makes its first attack, and of the course which it usually maintains. It might naturally be expected that a disease, so formidable as that which is produced by mercury appears to be, could not altogether have escaped



the notice of former practitioners, though they might have been mistaken as to its cause, and Mr. Mathias shows this to have been the case. Mr. Hunter's accuracy of observation could not overlook such remarkable effects as those which are detailed in this work; and he accordingly stated many instances in which they took place to a great extent, and in which he was fully aware of a deviation from the usual course of things, with respect to the operation of mercury on the system. He even went so far as to suppose that the mercury itself was concerned in the production of the symptoms: but it seems that he went no farther, and did not arrive at the result which the present author attempts to substantiate, that the mercury alone, without any united operation of the syphilitic poison, generates a separate and uniform disease. Observations similar to those of Mr. Hunter were made by Dr. Swediaur, and by Mr. Howard: but they in like manner stopped short, without reaching the important conclusion which is so explicitly brought into view by Mr. Mathias.

The great point which it behoved the author to establish, and which must serve as the basis of all his theoretical and of a considerable part of his practical reasoning, is the fact that the specific disease, which is described in this work, can be produced by mercury independently of the conjunction or interposition of any other morbid affection. It happens, however, that cases proper for the establishment of this position do not frequently occur, because it is seldom that mercury is taken in a sufficient quantity to excite its appropriate symptoms, without some other disease being present, which might be supposed to have a share in their production. Yet instances of the simple effects of mercury are occasionally found, and two are here detailed which appear to be fair examples of this kind; in which a train of symptoms ensued, both local and general, corresponding with the affection specifically styled by the author, *the mercurial disease*. At the same time that we give this opinion, we think that it would be extremely desirable to place so very essential a point beyond the reach of the slightest uncertainty; and we hope, now that the subject is explicitly brought into discussion, that more occurrences of

this kind will be noticed. It is a singularly embarrassing circumstance, and one which has principally tended to keep the matter so long in obscurity, that the mercurial disease, in all its leading symptoms, very strongly resembles syphilis; and since it is in a great majority of cases produced by the mercurial course which is adopted to remove syphilis, those appearances which should have been attributed to an excessive quantity of this medicine, have been ascribed to a deficiency of it. It is farther remarkable, and in a practical point of view extremely material, that, as soon as mercury begins to excite in the constitution its particular morbid effects, its antivenereal powers cease, and it has no influence over this complaint, until the constitution becomes entirely relieved from its *mercurial* effects. Proceeding on this hypothesis, every practitioner must immediately come to the conclusion, that a very large proportion of all those inveterate and deplorable cases, which were supposed to be syphilitic, but which mercury was found insufficient to remove, were really cases of 'the mercurial disease,' succeeding to an excessive or improper use of mercury; in which the venereal virus was either extinguished, or at least in a dormant state; and which were to be relieved, not by continuing the use of mercury, and even pushing it to a greater extent, but by entirely laying it aside. When we reflect on these facts, for facts they certainly appear to us to be, we shudder at the imminent danger in which the lives of so large a proportion of our fellow creatures have been placed; and we can scarcely feel sufficiently grateful to the sagacity and assiduity which have developed and established such important truths.

After having taken a general view of the disease, Mr. Mathias describes more minutely the local symptoms which it exhibits, the bubo, the chancre, the affection of the throat, and of the bones. We shall in general observe, on this part of the work, that the author expresses himself with clearness and precision; and so far as internal evidence can enable us to give an opinion, we should regard his account of the different forms which the disease assumes, as a faithful representation of what has actually passed under his observation. We before



remarked that the mercurial disease attacks all the parts which are the seat of syphilis; and it attacks them nearly in the same order, and assumes very nearly the same external appearances. The only mode, by which they can be distinguished, is by minutely attending to the previous history of the disease, to the effect of remedies and particularly of mercury on it, to the probable exciting causes, and to the progress which it is making. By the assistance of all these circumstances, a tolerable judgment of the case may in general be formed: but still many instances must occur in which the only deciding point will be to observe whether mercury retards or promotes the cure, and whether, when mercury has been given without effect, the symptoms are not alleviated by intermitting it.

As in the discrimination between the mercurial and the venereal affections, we are frequently obliged to assist our judgment by inquiring into the causes which operate in the production of the former, it is of great importance to make ourselves intimately acquainted with these causes. Mr. Matthias accordingly devotes a section to this subject, and enumerates at some length those which he conceives to act the most powerfully in this way. The causes which he states are partly constitutional, and partly local or accidental. A peculiarity of habit sometimes exists in which mercury acts as a poison, and almost immediately tends to the production of the mercurial disease, when given even in the smallest quantity. A plethoric state of the body, great nervous irritability, and the presence of scrofula or scurvy, seem also to favour this affection: but it is more frequently to the accidental causes that it owes its origin; and of these the most material are improper treatment during a mercurial course, the topical application of stimulating mercurial preparations to venereal sores, and the employment of the saline preparations of mercury instead of the simple pill or ointment.

The outline which we have given of the author's doctrine and opinions will render it sufficiently apparent, that a new light has here been thrown on the management of mercury and the treatment of syphilitic complaints; that in some instances, in which we were before involved in difficulties, we

have now a clear principle to direct us; and that a considerable change in our practice must be the result. A section is allotted by Mr. Mathias to this point; the great object of which is to show the importance of guarding against the exciting causes of the mercurial disease, and to intermit all mercurial applications as soon as it makes its appearance. We have afterwards some very judicious remarks on the operation of other supposed antivenereal medicines, mezereon, sarsaparilla, guaiacum, &c. The general conclusion is that they have no power over the syphilitic virus, but that they may be of use in removing the *mercurial disease*; and by an attentive examination of the cases in which they have been supposed to be serviceable, Mr. Mathias's opinion is rendered extremely plausible. It is highly probable that, in these instances, the syphilitic affection was subdued, and the mercurial disease alone remained. In this part of his work, the author has displayed a considerable share of acuteness and discrimination.

The last section treats on the cure of the mercurial disease; which consists fundamentally in refraining from the use of mercury as soon as the least indication of the existence of the disease takes place, and, which is one of the first circumstances to be noticed, as soon as the mercury ceases to produce any farther benefit with respect to the syphilitic symptoms. If the system be plethoric, the antiphlogistic regimen is to be pursued in all the different ways; if it be in an irritable or reduced state, we are to give a proper quantity of nutrition, and cautiously to administer tonics. Excepting occasionally, when topical applications are necessary, the above treatment appears to be all that is essential.

In concluding our remarks on this volume, we must express the great pleasure and improvement which we have derived from the perusal of it; and we have no hesitation in saying that it must produce an important change in practice, and must tend to facilitate the cure of some of the most unfortunate states of disease to which the human body is subject. As an addition to the other merits of the performance, we must not omit to mention the candor with which the author speaks of those who differ from him in opinion; a conduct which displays an amiable disposition and a cultivated understanding.



[From the Edinburgh Review, for November, 1810.]

*A Letter on the Differences in the Structure of Calculi, which arise from their being formed in different Parts of the Urinary Passages; and on the Effects that are produced on them by the internal Use of Solvent Medicines, from Mr. William Brande to Everard Home esq. F. R. S.* (From the Philosophical Transactions for 1808, Part II.)

*Some Observations on Mr. Brande's Paper on Calculi.* By Everard Home esq. F. R. S. (From the same volume.)

*An Account of a Calculus from the Human Bladder, of uncommon Magnitude.* By Sir James Earle, F. R. S. (From the Phil. Trans. for 1809, Part. II.)

*Observations on the Effects of Magnesia in preventing an increased Formation of Uric Acid; with some Remarks on the Composition of the Urine.* Communicated by Mr. William T. Brande, F. R. S. to the Society for the improvement of Animal Chemistry, and by them to the Royal Society. (From the Phil. Trans. for 1810, Part I.)

*On Cystic Oxide, a new Species of Urinary Calculus.* By William Hyde Wollaston, M. D. Sec. R. S. (From the same Work, Part II.)

THESE curious and valuable papers relate to one of the most excruciating torments with which the Divine Providence has, for wise purposes, decreed that the lot of man should be mingled, during his existence in this world. Indeed, were it not that custom teaches us to consider mental afflictions as, in the general, more severe than any bodily pains, (and, no doubt, to some natures, they are beyond all comparison, more unbearable,) we should be disposed to rank the dreadful complaint which we are about to treat of, as the worst of human maladies. To investigate the nature of the Stone, for the purpose of discovering solvents which might remove it, has accordingly been long considered as one of the noblest problems in practi-

cal chemistry, and among the best services which that science could render to the healing art. It may be remembered, that to this investigation we owe the brilliant discoveries of Dr. Black, which, by disclosing the nature of fixed air and of the alkaline earths, paved the way for all the modern improvements in chemical knowledge. Those great acquirements in speculative science were the compensations which he obtained for his disappointment in a search far more important and interesting to humanity: as the navigators of the fifteenth and sixteenth centuries discovered the New World, and explored its strange recesses, while occupied in the pursuit of an earthly paradise, or a fountain of health and youth. Even the legislature of this country took some part in those inquiries, and gave rewards to the discoverers of solvents. The most noted instance is that of Stephenson; and here the money was granted upon the faith of a cure *said* to be performed, the body not having been examined after death. We need scarcely add, that medicine thus patronized, and, as it were, privileged, has long since been completely exploded; and though, in its failure, the parliament resembled the celebrated philosopher whose name we have mentioned, we do not remember that its investigations were like his, rewarded by any other discoveries.

The hopelessness of the search for *solvents*, has lately turned the attention of the chemists and physiologists to another problem; and they have been occupied in ascertaining the process of calculous formations, with the view of administering such *preventive* medicines as may indispose the system to produce those concretions, or check their growth, without altering the tone of the constitution. The papers now before us throw considerable light on this important subject; and contain also a good deal of information, tending to elucidate the fallacies of the older doctrines respecting solvents. Indeed, did we not know the danger of trusting to medical reports of cures, and did we not recollect how regularly a series of cases, all quite apt and most nicely fitting the doctrines, forms a part of every modern treatise on medical subjects, we should be tempted to consider a great part of the question solved with regard to the stone, and should confidently look to the extirpation of that



worst of maladies. Even moderated by such unpleasant recollections, as we own our hopes to be, we cannot look to the eminent names prefixed to these tracts without great respect; and we feel at least so much inclined to listen, that we shall make no farther apology for laying before our readers an account of what the papers have added to the stock of our knowledge on this subject.

In order to convey some idea of the greatest degree in which this disease has ever yet, we believe, been known to afflict its wretched victims, we shall begin with noticing the case narrated in sir J. Earle's paper.

Sir Walter Ogilvie, a Scotch baronet, received, at the age of twenty-three, a severe blow on the back, from the boom of a vessel; and the pelvis and lower extremities were in consequence paralyzed. He remained in bed for fourteen months, in an horizontal position; and, during the first two months, his urine was of necessity drawn off by a catheter. After this, he had somewhat recovered the use of his limbs, and could walk or ride, though with difficulty: his general state of health, too, was weak and precarious. About twenty years after the accident, he was afflicted with symptoms of stone; and upon examination, a calculus of considerable size was found to have been formed in the bladder. An operation was now recommended, but, unfortunately, delayed from time to time, although the patient's health constantly declined, and the irritations and pains in the bladder greatly increased. In this melancholy state he continued for eight years longer, when he became unable to make water in an upright position: he was obliged to place himself in an inverted posture, that the upper part of the bladder might become the lower; and as, even by this means, very small quantities of urine only could be voided, the irritation of endeavouring to make it became almost perpetual, and he was obliged to perform the above mentioned operation every ten minutes, with scarcely any relief. The principal remedies which he tried were *aqua calcis* and *uva ursi*. He used opium also to alleviate the pain; and this, adding to a naturally costive habit, rendered aperient medicines frequently necessary, which, again, must have increased

the irritations of the disease. At the age of fifty-three, thirty years after the accident, the spasms became so constant and so violent, that he resolved to have the operation performed; and the paralysis of the lower parts being deemed no obstacle to it, he was conveyed by sea to London, where sir James Earle and Mr. Cline consulted upon his truly lamentable case, about the end of July 1808.

Upon examining the lower part of the belly, a tumor was perceived, so large, that sir James Earle doubted its being altogether caused by the stone; but, on sounding, the instrument would not enter the bladder; and further examination showed, that the whole cavity was nearly filled with calculus. It was clear, therefore, that this could only be extracted, if it should prove soft enough to admit of being broken, and thus brought away piecemeal; and it was deemed impossible to attempt the operation above the *os pubis*. The ordinary method was therefore resorted to; and after some days' consideration, sir Walter Ogilvie, well aware of the difficulties of the case, and of its great uncertainty, yet justly weary of the long life of torment which he had led, was resolved to have the operation performed, for the chance which it afforded of bringing a relief, only otherwise to be attained by death. It was accordingly performed by Mr. Cline, on the 11th of August.

He was placed in the usual position, and the ordinary ligatures were applied; but the paralysis of the lower limbs rendered it unnecessary to confine them, and they were left at liberty. It was found impossible to pass the staff, or any kind of forceps, into the bladder; but on pressing hard with the finger, part of the stone felt soft, and gave way. This making room for the forceps and scoop, some parts of the concretion were brought away; and, in this manner, about a large teacupful was extracted. The great mass, however, continued hard and immovable: it could neither be broken nor shaken from its position; and after trying every instrument, of all powers, the operator was compelled to desist, leaving it (with the exception of the soft matter brought out) in its former state. The patient bore this long process with great fortitude: happily, the paralysis of his lower regions may be supposed to have some-



what diminished the sensibility of the parts. He was, however, completely exhausted, from weakness and fatigue.

Although no hemorrhage, nor any fatal symptom, appeared to result from the operation, it had afforded no sort of relief; and the spasms continued as before. They soon increased in frequency and effect, as he became exhausted, and unable to stand them. He seems to have had a gleam of hope on the morrow of the operation; for he frequently inquired when he might be well enough to have the remainder extracted. But he gradually sunk under the repeated and violent irritations of the spasms. On the eighth day, his pulse became smaller and quicker, and he could scarcely be prevailed on to take any sustenance: on the ninth, he was more impatient, feverish and restless; and on the tenth day, "he desired not to be teased to take any thing more; when covering himself completely with the bed-clothes, he quietly resigned a most singularly miserable existence."

On dissection, the bladder was found to be *quite filled* with a huge calculus, composed of the triple phosphate, which we shall afterwards have occasion to speak of; and of such an unusual proportion of animal matter, that it speedily showed a strong tendency to putridity, and even engendered maggots. It weighed forty-four ounces, or three pounds four ounces, apothecary's weight. Its form was that of an irregular ellipsoid; the larger circumference being sixteen inches, and the smaller fourteen. It had filled the bladder, and taken its shape, but apparently without distending it. The ureters had become enlarged, so as to contain the urine, and act as small bladders; while the large bladder only permitted that fluid to pass off by means of small furrows which it made in trickling along the surface of the stone, between the stone and the bladder. There appears no reason to doubt, that, in a short time, even this painful and insufficient operation would have become impracticable, by the further enlargement of the stone stopping up the mouths of the ureters; in which event, death must have ensued from suppression.

Such, in its very worst state no doubt, is the dreadful malady which forms the subject of the chemical papers now be-

fore us. To these we must now direct our attention; and we can promise the reader sufficient reward for his trouble, if he shall be pleased to follow us through the analysis of their contents.

Mr. Brande's valuable paper, which stands first in the prefixed list, is founded on an examination of the collection of calculi contained in the Hunterian museum. Beside the known richness of that collection, most of the specimens have histories of the case annexed to them, which are obviously of the greatest use in the investigation of the subject. Mr. Brande arranges calculi into four classes: first, Those which are formed in the kidneys, and voided without undergoing any change in the passage; second, Those which are retained in the kidneys; third, Those which are found in the bladder; and, fourth, Those which are found in the urethra. On each of these species, our author has made a number of experiments; and the results exhibit a sufficiently distinct account of their chemical analysis. The first class, or calculi of the kidneys, consist almost always of uric acid (the Lithic acid of Scheele) and animal matter; the animal matter is found in various proportions, from one-eighteenth to one-third of the whole compound. Sometimes the acid and the matter are not formed into a solid calculus, but pass off mixed with phosphates. The author, however, is disposed to think that the phosphates are not elaborated in the kidney, but mingle with the calculous matter in its passage. In some few instances, the calculus contains oxalate of lime; but this is very rare. The size of these calculi varies, and one has been described by Dr. Heberden, so large as to weigh twenty-eight grains; but it may have been detained on its passage, and so augmented in bulk. The largest which Mr. Brande examined only weighed seventeen grains. All the calculi of this class are certainly soluble in pure potash; but it is needless to add, that the caustic qualities of this substance render its application in the living subject impossible.

The second class is formed by the detention of a calculus in the kidney, while either the secretion of uric acid enlarges its bulk, sometimes so as to form a complete cast of the pelvis of the kidney; or the stream of urine to which it is exposed deposits the phosphates upon the original nucleus of uric acid and



animal matter. In the one case, we have an enlarged calculus of the first class: in the other, a calculus of the third class, only detained in the kidney.

When the uric calculus passes into the bladder, and is there detained, or when a calculus of oxalate of lime comes down from the kidney; or when portions of sand or animal mucus are deposited in the bladder; or when any extraneous substance has found its way thither and is detained—in all these cases, that is, on all these substances as nuclei, there are formed calculi (commonly termed stones in the bladder) with greater or less rapidity, according to the tendencies of the constitution. They may be ranged into three subdivisions: those composed chiefly of the uric acid; those chiefly composed of phosphates (the ammoniaco-magnesian phosphate, or phosphate of lime, or a mixture of the two;) and those containing oxalate of lime, which are commonly termed mulberry calculi. Messieurs Fourcroy and Vauquelin, and Dr. Pearson, having found the greater number of calculi to be composed of uric acid (that is to say, belonging to the first of these subdivisions), our author was surprised to find that, in the Hunterian collection, this proportion did not hold; for, of a hundred and fifty which he examined, only 16 were composed of uric acid wholly (beside the animal matter:) 45 of that acid with a small portion of phosphates: and 5 of the acid with phosphates and nuclei of oxalate of lime; while 66 contained phosphates with a small admixture of uric acid; 12 the phosphates entirely; and 6 chiefly oxalate of lime. Observing that a greater loss was sustained in obtaining uric acid from calculi in the bladder, than from calculi in the kidneys, Mr. Brande was led to conclude that the former contained urea. With a view to ascertain this point, he made some experiments, and found that an uric calculus, weighing 60 grains, contained 5.2 of urea and muriate of ammonia, 6 of ammoniaco-magnesian phosphate, and 48.8 of uric acid: but no urate of ammonia could be detected: from whence, and from other experiments, our author infers, that this substance, when supposed to exist in calculi, owes its origin to the development of ammonia, and its recomposition with uric acid in the course of the processes of decomposition. The mulberry

calculi (which are rare) contain, in general, a considerable admixture of phosphates and uric acid with the oxalate of lime. The very large calculi often contain an agglomeration of different species of calculi. Mr. Brande examined one, of twenty-three ounces and twenty-six grains, which had a nucleus of uric acid, surrounded with a considerable quantity of very pure ammoniaco-magnesian phosphate: the mass of the stone consisted of mulberry calculus. Another, weighing  $15\frac{1}{2}$  ounces, had a nucleus of uric calculus, round which was a mass of ammoniaco-magnesian phosphate, intersected with laminæ of uric acid. It is rare to find calculi composed of four distinct substances. Our author saw one which had four separate layers, or strata, of uric acid, oxalate of lime, ammoniaco-magnesian phosphate, or phosphate of lime. He examined four calculi, the nuclei of which were extraneous substances, viz. a garden pea, a needle, a hazel nut, and part of a bougie. Besides the animal matter, the first calculi contained phosphates only; the second, phosphates and oxalate; and the other two were also destitute of uric acid.

The fourth class of calculi, those found in the urethra, offered no peculiarities to the observer. Indeed, the classification of Mr. Brande does not refer so much to any distinct properties of the substances arranged, as to their position and accidents; their *habitat*, as the botanists express it; for their properties seem to depend merely upon their exposure to the urine in the different stages of its elaboration; nor is there any manifest variety of circumstances affecting their formation, except in so far as they are found in streams of that fluid in its nascent or in its perfect state. The calculi in the urethra, accordingly, are only small calculi which, passing from the bladder, have been detained and lodged in the membranous part of that canal, and, there, have been exposed to the same process of slow accretion, from the passage of urine, which they would otherwise have undergone, from lying in the bladder exposed to the action of the same fluid. One of the calculi of this description, which Mr. Brande examined, was of a singular appearance. It was a perfect sphere, of about half an inch in diameter, coated with



small regular crystals of the ammoniaco-magnesian phosphate in its purest state.

Our author closed his experiments with examining the calculi of different animals. In the horse, sheep, rhinoceros and rabbit, the calculi consisted of the phosphates in part, with animal matter and carbonate of lime. In the dog, no carbonate of lime was found; and, in the hog and ox, no phosphates; the calculus being wholly composed of carbonate of lime and animal matter. No uric acid nor oxalate seems to have been detected in any of the specimens. Some of them were of a large size: a calculus from an old dog weighed sixteen ounces.

From this indication, it is very evident that one conclusion may safely be drawn: that, in most instances, the stone is connected with the formation of uric acid. Sometimes that acid forms calculi in the kidneys, which after creating sufficient torment in that region, pass into the bladder, and become the nuclei of still larger, more painful, and more fatal accretions of other substances; while, in many instances, the same acid continues to augment the mass, either alone or combined with other substances. To prevent its formation, then, becomes a most important object; and both Mr. Brande and Mr. Home propose, for this purpose, the exhibition of some alkaline earth, which may neutralize and carry it off. But, before adverting to this preventive remedy, we may notice the additional proofs adduced, in the papers now under review, to evince the hopelessness of any attempts to solve the stones already formed, at least in the present state of chemical science.

In arguing this point, Mr. Brande (or, as Mr. Home always terms him, Mr. W. Brande) proceeds more by principle; his patron Mr. Home comes forward to his protection with cases. Mr. Brande clearly shows, that an alkali administered to a calculous patient stands no chance of reaching the uric concretion in a caustic state; for the urine contains both phosphoric and carbonic acid uncombined. But experiment clearly shows, that neither carbonates nor subcarbonates exert any sensible action on uric acid: in other words (as we apprehend), the affinity of the uric acid for alkalies is weaker than the affinity of carbonic acid for the same bodies: therefore, alkaline

liquors cannot act as solvents of the uric calculi. On the other hand, if acids be administered with a view of attacking the phosphates, the formation of the uric acid is greatly favoured. This, Mr. Brande's experiments have also taught him. And as for the injection of solvents through the urethra into the bladder, he justly observes, that although we were not ignorant (as we are) of the kind of calculus in each case, the frequent introduction of instruments into the bladder, and the long continuance of the operation, would form insuperable obstacles to this mode of treatment, which, though recommended by great names, has always been speedily relinquished when tried.

Now come Mr. Home's cases: which plainly demonstrate, that where the use of alkaline medicines was supposed to have relieved the patient by dissolving the calculus, on examination no such effect was found to be produced. In two cases, where the violence of the complaint appeared to have subsided, the dissolution was taken for granted, and ascribed to alkaline medicines; but, on dissection, the calculi were found in great size, only imbedded in cysts, from the enlargement of the prostate gland, which often takes place late in life. In some instances, again, the increase of calculous concretions was found to proceed rapidly, while the patients were going on with courses of alkaline medicines. One person took these remedies for four or five years, and, at his death the bladder was found nearly filled with light, spongy calculi of different sizes, no less than 350 in number. Another, who had taken soda, both mild and caustic, for some months, and then submitted to the operation on the symptoms increasing, was found to have a calculus, which was surrounded with a coat of triple phosphate, one tenth of an inch thick, the rest being a mixture of uric acid and phosphates; from whence it is fair to infer, that the alkali increased the formation of triple phosphate, though it checked the production of uric acid. In a third case, of a very virulent nature, the exhibition of alkalies did not even prevent the formation of uric concretions. Mr. Brande's experiments however, in 1808, having rendered it highly probable that, in the great majority of cases, alkaline medicines would operate powerfully in checking the growth of uric



calculi, by neutralizing the uric acid before it could form a concretion, this inquiry has lately been resumed by that industrious and skilful observer, with the aid and advice of Mr. Home; and the result of their joint labours is given in the fourth of the papers examined at the head of this article.

Upon consultation with Mr. Hatchett, they were led to fix upon magnesia, in preference to the other alkaline substances; and the event appears to have justified this very natural and judicious choice. Some preliminary trials showed, that the mild, safe, and easily prepared earth in question, diminished the quantity of uric acid in urine, which contained a larger proportion of it than any of the alkalies, however copiously administered. But this point is of such primary importance, that we must notice the four cases in which a further examination of the matter was prosecuted.

The first was that of a gentleman sixty years old, who, apparently from the habit of drinking acid liquor, had acquired an uncommon tendency to secrete uric acid, and had his urine constantly mixed with that substance, in the form of red sand or crystals. He took, first, the subcarbonate of soda, and then of potash, without any amendment from the former, and with but a slight relief from the latter. His urine being previously examined, he was ordered to take fifteen grains of magnesia three times a day, and in a week the uric concretions diminished sensibly: in three weeks, they were only observed occasionally. The same medicine has been continued for eight months, and no calculi have been voided; nor has there been any material deposit in the urine. He has also been cured of heartburn, and other stomach complaints. Another gentleman, of about forty years of age, who suffered greatly from a similar complaint, and tried the alkalies in vain, has been wholly cured by a six weeks' course of magnesia, in the quantity 'of twenty grains every night and morning,' (we wish Mr. Brande would say more distinctly whether he means *twenty* grains a day, or *forty*), without any change of regimen whatever. The third case, is that of a gentleman forty-three years old, who has, for about a year, succeeded in driving away repeated attacks of the uric acid, by taking magnesia for a few

weeks at a time, when the fit comes on. The fourth case is that of a more confirmed calculous tendency, which has nevertheless yielded, in a great degree, to the free use of magnesia; with this addition, that since the patient began it, he has been free from gout, contrary to his former habits.

The different qualities of magnesia and the alkalies thus observed, led our author to make some experiments on their respective effects upon *healthy* urine. Subcarbonate of potash and soda occasioned a copious precipitation of the phosphates in the urine, during the first two hours after these salts were taken into the stomach; and, after that, no further effect was produced. The same alkalies, saturated with carbonic acid, threw down the phosphates less copiously and rapidly. Magnesia, administered in the same circumstances, produced a much smaller and slower precipitation. Instead of this effect reaching its maximum in a quarter of an hour, as it did in the case of the alkalies, it did not reach that point in less than six hours; and on this circumstance our author concludes, that its value in calculous disorders chiefly depends. Lime, either in the caustic or mild form, produces a very slight effect; and its nauseous taste, as well as difficulty of administering it in sufficient quantities, manifestly preclude its use in this disease. In one case, carbonic acid, exhibited in aerated water, was found to keep the phosphates (which the urine was greatly charged with) in a state of solution; and, when left off, those salts were again voided in their solid shape.

Thus far, then, we think, it may safely be hoped, at least with as little risk of disappointment as usually attends our speculations in medical science, that some light has been thrown on the method of treating a disease, of all others which prey upon the body, the most inimical to the happiness and comfort of man. Where the object is so valuable, it is natural for us to feel uncommonly anxious and distrustful, as well as more than usually eager in our wishes for its accomplishment. Instead, therefore, of being satisfied with what Mr. Brande and Mr. Home have already effected, we would expect them to persevere in their experimental inquiries: multiplying the number of their observations on real cases: and, as it were,



leaving no stone unturned to attain the complete mastery of this great problem. Much will be done for medicine, if they should go no further than perfecting the preventive application of magnesia to the uric calculus. A great number of calculous cases, of the worst description, will be prevented: those, to wit, in which the stone in the bladder is formed on a uric nucleus. Many others of the same class will be greatly relieved. Those, in which composite calculi consist partially of uric acid; and all that class of complaints with which the region of the kidneys is afflicted, by the uric concretions formed there, will be entirely removed. Should such a step in the healing art be made, we need scarcely despair of living to see the phosphates themselves attacked by it, and yielding to some equally simple and safe remedy.

While we are, however, indulging in these visions, and anticipating the final eradication of all calculous disorders, we unluckily cast our eye on the last of the papers which are now before us, and find, that Dr. Woollaston has been discovering a new species of urinary calculus, at the very moment that his learned brethren were occupied in extirpating the already sufficiently numerous specimens of the old catalogue. This new species is extremely rare, our author having only met with it twice. It resembles the triple phosphate more than any other kind; but is more compact, and consists, not of laminæ, but of a confused mass of crystals, having a yellowish lustre and semitransparency; but it is formed of a peculiar substance. Dr. Woollaston made as many experiments on this substance, as the limited quantity of it in his possession would allow. When burnt, it gives a smell quite peculiar to itself. When distilled, it gives a fetid carbonate of ammonia, and an animal oil, leaving a spongy coal. It is readily dissolved by all the pure alkalis and by lime water: it is even solved by the carbonates of potash and soda. The acids dissolve it also, except the citric, tartaric, and acetic. Neither water, alcohol, nor saturated carbonate of ammonia, dissolve this singular substance. From its disposition to unite with both acids and alkalis, Dr. Woollaston suspects it to be an oxide; which is confirmed by its forming carbonic acid in distillation; that is to say, if we take it for granted, as he seems to do, that this acid

does not exist ready formed in the mass. If it does contain oxygen, our author admits, it must be in a quantity insufficient to give it acid properties; for it produces no effect on the color of litmus paper. On the supposition of its being an oxide, and to distinguish it from other bodies of that class, Dr. Woollaston proposes to name it the *cystic oxide*, on account of its having hitherto been only found in the bladder.

This excellent chemist concludes his paper with some curious observations on the connexion between the production of uric acid in birds, and their food. The white matter contained in their urine, and voided along with their dung, was found by M. Vauquelin to consist principally of that acid; and our author examined with some care in what manner its quantity was affected by the diet of those animals. In the dung of a goose fed on grass, it only formed  $\frac{1}{200}$  part; in that of a pheasant fed on barley, it amounted to  $\frac{1}{14}$ ; in that of a hen which fed on insects as well as vegetable food, in a barn-yard, it was much more abundant, and mixed with lime. The solid part of the dung of a hawk fed wholly on flesh, was chiefly uric acid; and the evacuation of a gannet, feeding solely on fish, consisted altogether of urine; for the only solid parts were uric acid. 'It seems, (our author concludes) deserving of inquiry, what changes might be produced in the urine of any one animal by such alterations of its diet as its constitution would permit; for, as far as any inference can be drawn from these varieties which naturally occur, it would appear that persons subject to calculi consisting of uric acid, as well as gouty persons in whom there is always a redundancy of the same matter, have much reason to prefer vegetable diet; but that the preference usually given to fish above other kinds of animal food, is probably erroneous.'

The mention of Dr. Woollaston's paper naturally leads us to reflect on the important services which this truly philosophical inquirer has formerly rendered to the branch of science now under consideration. It was his important discovery of the nature of gouty concretions, which first ascertained the analogy long suspected between gout and stone; and, besides giving the first analysis of those concretions which had ever



been experimentally attempted, he also first (with the able assistance of Mr. Tennant, a name far too well known to need the humble tribute of our admiration) investigated the composition of the calculi, consisting of neutral salts. The discovery of Scheele, perhaps his most important discovery, brought to light the nature of uric calculi; and, at the same time, made us acquainted with a new acid. The existence of this acid was suspected in gouty concretions; and it was generally imagined that they consisted of the acid alone, or combined with animal matter only. Dr. Woollaston showed by experiment that they are composed of uric acid and soda. Until he carried his inquiries into the other classes of calculi, it was only by conjecture or vague analogies that their constitution was known; but to him we owe as perfect an acquaintance with their component parts, as Scheele had left us of the uric calculus. Having thus completed our analysis of the calculi formerly known, he has, in his present communication, brought to light both a new calculus, and a body. His former paper was published in the Philosophical Transactions for 1797; and the one now before us forms an important appendix to it. It is not within our province to take any further notice of the first tract; but we could not conclude this article without recalling it to the recollection of the scientific reader, and reminding him of his great obligations to Dr. Woollaston.\*

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*Description of an affection on the Tibia induced by Fever, with Observations on the Treatment of this Complaint.* By THOMAS WHATELEY, Member of the Royal College of Surgeons in London. 8vo. London, 1810. pp. 59. Colored plate. Callow.

From the Medical and Physical Journal, for December, 1810.

THE complaint which this essay professes to describe, is a disease of the tibia. It is pointed out by one or more small openings in the integuments lying upon the surface of this bone, through which an instrument may be passed into its cavity. A

\* See also a valuable collection of historical notices, as well as analytical researches, on urinary concretions, in Dr. George Pearson's paper, Phil. Trans. 1798, Part I.

proper examination with the probe will detect one, or sometimes several loose pieces of bone lying in this cavity; and which being larger than the external opening, are necessarily confined within the tibia, and being dead matter, certainly and invariably prevent the healing of the ulcer.

The curative process is simple and effectual. The external opening must be enlarged to a dimension that admits of the exfoliated portion of bone being removed; and then with easy dressings, a proper attention being given to the general health, and care taken to avoid the causes of irritation, the ulcer soon heals.

The only question seems to be how this opening into the cavity of the tibia is to be enlarged, for the purpose of extricating the portion of dead bone. Most surgeons would use the trephine; Mr. Whateley, however, prefers caustic. (*Kali purum.*)

This disease, which is undoubtedly an internal exfoliation of the tibia, is, perhaps, generally a *sequela* of fever; or, is some way connected with, or excited by, a preceding febrile affection.

"Having observed," says Mr. Whateley, "that in almost all the cases of this kind, which I have had in hand, the complaint had been preceded by fever, I have been led to consider the disease in question as a febrile affection of the part. I should inform the reader, that the case, now before him, is not one which frequently occurs. I do not recollect having, in the course of my practice, had more than about thirty of them intrusted to me. But having, in the course of many years, observed a certain peculiarity in the disease, I took notes of most of the cases, which came under my notice, conceiving that I might, at some future time be thereby enabled to afford useful information to the public, on the treatment of this complaint.

"These cases occur more frequently at the middle period of life, than at any other.\* In most instances, that came within my observation, the preceding fever was of considerable duration; and sometimes so violent, during its continuance, as to

\* See next page for note.



confine the patient to his bed, and often to be attended with delirium. Some time after the patient's recovery, he was seized with pain in one of his legs. The period at which this pain commences, is not the same in every case. In some patients, it takes place immediately on the termination of the fever; in others, it is not experienced till a few days afterwards; in others again, not till several weeks, and in others, not till many months have elapsed. Yet I should observe, that I have always found the disease to make its attack, within twelve months from the termination of the fever."

The phenomena and symptoms of the complaint are described with minuteness and precision.

"In most of the cases, the pain in the leg was acute, and appeared to the patient to proceed from the interior of the *Tibia*. The pain, indeed, was not equally violent in every case; as some of the patients were confined to their bed for several days, or weeks, and deprived of sleep for a considerable time; while others were able to go about. No external swelling or inflammation is, in general, to be expected, immediately on the commencement of pain. Yet these appearances are seldom long in following; they are usually visible within a week, and sometimes in two or three days. In some cases, a general inflammation and swelling over the whole limb first appeared, and soon after a more circumscribed inflammation, near the affected part of the *Tibia*, in which a fluctuation of pus might be felt. In other cases, the inflammation and swelling never extended to the entire limb, but were, with the subsequent formation of pus, confined to a small circle on the *Tibia*. In

\* I took notes of twenty-two cases which have been under my own care, the ages of these are as follow:

Of the age of 14	-	-	1
18	-	-	1
19	-	-	1
Between 20 and 30	-	-	7
30 and 40	-	-	11
40 and 50	-	-	1
			22

most instances, the suppuration was confined to one part of the tibia; and this was often about the middle of the leg, and the centre of the bone. In many cases, however, the suppuration took place in other parts of this bone: namely, within a few inches of the instep, a little below the knee, or in some of the intermediate spaces. Sometimes it has happened, that two or more distinct suppurations, and as many separate openings into the cavity of the bone have taken place. These were sometimes found within an inch or two of each other; at other times further apart. There was some variation likewise, as to the time of their appearance: some weeks elapsing between the commencement of each suppuration, in some cases, while in others, all appeared nearly at the same time.

“Although a fluctuation of pus might be discovered, within a short time after the disease had taken place, the matter did not usually burst through the skin as hastily as it does in some other suppurations. This discharge, however, sometimes took place within a week or two, from the first appearance of matter; but in many cases, the pus continued in a confined state, for many months, and required the use of the lancet to set it free.\*

“In most of the cases, the violence of the pain ceased: the inflammation and swelling abated, as the fluctuation of matter was felt; and in a short time afterwards, the patient was generally able to walk about. Soon after the discharge of matter, the wound usually contracted to so small a size, as scarcely to admit the point of a probe; and a small quantity of pus continued to ooze from the aperture. In some cases, however, the ulcers closed up for a few days, and then burst out again. If in this state of the disease, a probe is passed carefully into the little orifice of the ulcer, it usually enters readily, into the cavity of the tibia,† where the loose piece or pieces of exfo-

\* In the greater number of cases, the suppuration is small, often containing not more than a tea-spoonful or two of pus. In other cases, however, I have seen a larger suppuration, in which, an ounce or more, of matter is formed.

† In one or two cases, I have perceived this orifice so small, as not to admit the round end of a probe, of the usual size. In a few other cases, it has been so oblique, as to be entered with much difficulty by that instrument.



liated bone are frequently to be felt. Under these circumstances, I have never known a perfect cure to take place, unless where the appropriate treatment had been resorted to.

"In this disease, the surface of the tibia, immediately around the orifice, becomes often, though not always, knotty and irregular; and the periosteum is sometimes thickened, so as to give an appearance, resembling that of a venereal node. This disease, however, differs totally from those affections of the tibia produced by syphilis, as it does likewise from those usually denominated necrosis, or from any of those which are produced by scrofula. In short, as far as I have been able to observe, it is a disease *sui generis*, the relic of fever, usually affecting the tibia.\*

"From the violence of the pain, and sudden erosion of the bone, an inflammation appears to attack its internal part. Suppuration takes place; and matter is first formed there, and then makes its way through the substance of the bone, to the outer integuments. As, however, the ulcerative process in the bone sometimes takes place in different parts, thereby producing several apertures in the tibia, matter is, in these cases probably, formed in distinct places, previous to the ulcerations in the bone. One of the most remarkable circumstances attending these cases is, that one or more pieces of loose bone are usually found within the cavity of the tibia, opposite to each aperture.

"Some of these exfoliations are so small and thin, as to require a nice examination with the probe, in order to discover them. They are, however, generally found of a larger size, sometimes even exceeding an inch in length. I have always found these exfoliations of an oblong and spiral shape, and evidently separated from the internal laminae of the tibia within the cavity of the bone. The separation of such a portion of bone is probably the effect of the previous inflammation and ulceration; by which it is deprived of circulation, and of course

\* I have some recollection of having, many years ago, seen this disease in the fibula; but in that case, the tibia was likewise affected.

loses its connexion with the living bone, of which it was once a part."

Having inquired into the probable cause of this disease, and passed through the various appearances it assumes, the author proceeds to describe the curative process.

"During the painful and inflammatory state of the disease, and before the erosion of the bone has taken place, it would be proper to apply emollient fomentations and poultices to the limb: to keep the patient in bed; and to endeavour to ease the pain, by occasional doses of opium. I must confess, however, that I have seldom seen the disease in this stage. I have commonly been applied to, after the erosion of the bone has taken place, and when the patient has been, in some measure, able to follow his usual employment. In this stage of the disease the cure of the ulcer, by the common external remedies, is impossible: the loose piece or pieces of bone, pent up within the cavity of the bone, must set that mode of procedure at defiance. The imprisoned exfoliations, therefore, must be extracted; and this is desirable to be done, by the mildest methods possible, avoiding all unnecessary pain and operations to the patient.

"The loose bone within the cancelli, is commonly longer than the ulcerated aperture in the bone. It is in many cases, indeed, considerably longer, and though confined to its situation, it may sometimes be taken hold of by a pair of forceps, and moved upwards and downwards, in its bony case. The examination by the probe should, therefore, be made with great care and gentleness; otherwise, much unnecessary pain will be excited. It may happen also, that the exfoliated bone may be removed from the favourable situation it is in, when it is directly under the ulcerated aperture, and be pushed under the arch of the tibia, as I have more than once experienced; by which the difficulty of extracting it must be greatly increased.

"As the orifice in the tibia, as well as the exfoliations, are of different sizes in different cases; and as they are generally covered by the integuments and granulations, it requires a nice examination with the probe, to ascertain the true state of the parts. In some cases, as before observed, the aperture in the tibia is so small, as not to admit the round end of the probe, to



pass into the cavity of the bone, and the contrary end, or an eyed probe, will pass into it with so much difficulty, that no accurate examination of the parts beneath can be made. In most cases, however, it freely admits the round point of a probe; and our attention should first be directed, to find out the size of the aperture in the bone. Having ascertained this, we should next examine, whether there be a loose piece of bone within the cancelli. The size of the aperture may be known, by passing the probe, in a gentle manner, from side to side in all directions. But it is not easy either to find out the exfoliated piece, or to ascertain its exact dimensions, when discovered.

“Where the aperture in the tibia is large, and the exfoliated piece very small, the latter may be moved about, with the end of the probe, so readily, as to leave no doubt of its easy extraction, by the introduction of a pair of fine forceps. Under such circumstances, the operation should be instantly performed; and if the whole be removed, the wound will generally heal up in the course of a few weeks: the application of any common dressing, aided by bandage, will be sufficient for the purpose. The utility, therefore, of first examining with great nicety, by the probe, in order to avoid, if possible, the use of more violent remedies, must be evident.

“In more difficult cases, the exfoliated piece may, likewise, be readily moved in the cavity of the bone, by the end of the probe; yet the hand of the operator will be sensible that it is confined, by the sides of the surrounding tibia. In some cases of this kind, the opening in the tibia may be large enough to admit of our taking hold of the exfoliation, by a pair of common forceps; but in others, it is so small, that a pair of the finest forceps that can be made, cannot be made to enter, so as to embrace the bone. When possible, we should endeavour to extract the exfoliated bone by the forceps; and in these attempts I have readily succeeded, when the exfoliation was not longer than the aperture in the tibia. But where it proved to be of greater length, which might be known, by its being moved upwards and downwards in its bony case, by the forceps, but whence it was not possible to extract it; I have been sometimes able to accomplish the extraction, by moving it

upwards or downwards, as far as it would go, and then raising one end of it. Where I could not succeed by this procedure, I have been sometimes obliged to resort to a more violent method; that of endeavouring to break the exfoliation into two parts, by means of the forceps. This I have been able to do, where it has not been very strong, and having thereby extracted it, the cure has been speedily completed.

"There are other cases, however, in which methods very different from these, must be taken. We may have the strongest reasons for concluding, that there is a loose exfoliation, within the cancelli, and yet may not be able to feel it by the probe. In this case, as well as when the opening in the tibia is too small to admit an exfoliation to pass, by any methods above described, we must apply the *kali purum* to the integuments around the ulcerated opening in such a manner, and in such quantity, as to destroy them, to the extent of about half an inch from the centre of the opening. This is done with a view to expose as much of the surface of the tibia, as is requisite for the cure.\* The proceeding, here recommended, is attended of course with some pain; and it requires particular attention, in order to prevent the *kali* from destroying more of the integuments than is necessary; and from penetrating through the orifice into the cancelli.

"This caustic may be applied in several different ways. That which I prefer is, to take about as much of it (bruised into parts of the size of a pin's head) as will lie upon a seven shilling piece, and apply it, both to the ulcerated opening in the skin, and to the surrounding integuments, to the extent already mentioned. Pieces of dry lint, or adhesive plaster, should be applied around the caustic, to prevent it from extending on the adjoining skin. In a few minutes, the *kali* liquefies, and begins to operate.

"The leg should be placed in a horizontal position; and if the caustic appear to act equally, the limb must remain in that position for about ten minutes, or a quarter of an hour; after

\* The orifice of the bone is situate so near to an angle of the tibia, in some cases, that it does not admit of its being exposed, to the extent of half an inch, on all sides of it.



which, a pledget of dry lint should be applied over the kali, large enough to cover the adjoining sound skin. The leg should then be slightly bandaged with a flannel roller.

"It is to be observed here, however, that during the action of the caustic, some additional attentions are not unfrequently required, in order to ascertain the depth to which it has penetrated. If in one part, the integument is thicker than in another, or if the caustic is acting more powerfully on one part than on another, a little of it may be removed with the probe, to or from such part; or a little more fresh caustic may be applied to any particular part, if it appear to require it. During the action of the caustic, particular attention must be paid to its effect on the little ulcer on the skin, and on the orifice in the tibia, as it is desirable to prevent its penetrating the cancelli, much harm and disturbance being likely to arise from its action on this part.

"If the integuments adjoining to the wound be thick, the kali may be applied to this part, nearly in the same quantity as to the other parts; as, in general, it ceases to act before it can penetrate into the cancelli. But if the integument, on this part be very thin; or if there be no integument or granulation over the ulcerated orifice, a small piece of lint should be passed down it to the cancelli, previous to the application of the kali, to prevent it penetrating too far: a piece about the size of a pea will generally be sufficient. This should be rolled with the finger and thumb, and firmly pressed down the orifice by a probe, till it passes into the cancelli, where it should remain. If this should not perfectly plug it up, a second, or even a third pledget should be applied. This is often attended with some pain, from the pressure made on the granulated flesh at the bottom of the wound, which generally springs from within the cancelli, and is exquisitely sensible. The kali should remain on the part to which it is applied, for about six hours. After this, the wound should be dressed twice a day with some dry lint, and an emollient poultice. In about a week or ten days, from the application of the caustic, the slough will come away.

"If the caustic have performed its office, the surface of the tibia will be perfectly bare; and now the unequal, knotty state

of the bone, and thickening of the periosteum, will often be seen; and if the granulations on the surface of the orifice have been destroyed by it, the loose exfoliation within the tibia will often be visible to the naked eye. But where the granulations are not destroyed, the internal exfoliation cannot be discovered without introducing the probe into the wound, as before directed.

“ Sometimes the surface of the tibia around the ulcerated aperture, will be found, on the separation of the slough, not to be sufficiently exposed. In this case, a little of the kali must be applied a second time, to such parts as require it. This should be done in the course of a few days, otherwise the wound will begin to fill up with granulations. Though a repetition of the caustic is seldom attended with as much pain as accompanies the first application, it being seldom necessary to destroy a fresh portion of the skin; it should, nevertheless, be applied with all the precautions, recommended in the first instance; the like dangers being to be guarded against, on the subsequent application as on the first. It must be remembered, however, that a proper quantity should be applied, otherwise the surface of the bone will not be sufficiently exposed, when the new slough separates. When the second application has been properly conducted, we seldom have occasion for a third.

“ The surface of the tibia, with the aperture into its cavity, being thus exposed to view, we proceed to search for the loose exfoliation, if it be not already visible. The orifice in the tibia must again be carefully examined by a probe, and as this examination can now be assisted with the eye, the size and situation of the exfoliated piece will for the most part be readily discovered. If indeed the granulations within the cavity of the bone shall not have been destroyed by the caustic, some impediment to the examination may arise from them, as they are in general extremely sensible, and are apt to bleed, on the slightest touch of the probe. But as the exfoliated piece generally lies buried in these granulations, and is perfectly detached from the contiguous bone, it may frequently be discovered, yielding to the pressure of the probe.

“ Its situation being discovered, and its size, compared



with that of the aperture in the tibia, pretty fairly ascertained, we must endeavour to extract it by the forceps; and sometimes we shall succeed, where the endeavours made, previous to the application of the caustic, have failed. The exfoliated bone, however, will often be found to be larger than the opening in the tibia, in which case it will, in general, be impossible to extract it, till the orifice has been widened.

“If the trephine be used for this purpose, it may be sometimes difficult, though probably never impracticable, to remove the piece incircled by the instrument after it has been worked to a proper depth. I remember to have succeeded many years ago by this method, in extracting a large piece of bone, pent up within the cancelli. The case to which I allude occurred to me when I was a very young practitioner, and has been already published. From the date of this operation, there has not occurred a single case in my practice, in which I have found it necessary to use the trephine. I have, for some years, practised with great success a method of enlarging the opening in the tibia, which is much more simple, as well as less painful. It is by means of the kali purum; and the way in which I use it is as follows: I apply this caustic to the bare surface of the tibia, around the hole, and to the sides of the hole itself, after the separation of the slough; taking care to guard against its touching the adjoining parts, or penetrating into the cavity of the tibia: each of which dangers may be prevented, by the application of the lint, as before directed. In about ten minutes after the application of the kali, the bone should be covered with a pledget of lint; and on the following day the kali should be again applied to all the parts, as before. After this the limb should be dressed twice every day with a fresh poultice; it should be lightly covered with a flannel roller, and the patient may be generally permitted to take moderate exercise. In a month or less from this period, an exfoliation of all that part of the bone, to which the caustic has been applied, will take place.\* This ef-

\* I have not known this exfoliation take place in less than three weeks from the application of the caustic; and it happens, not unfrequently, that it is not separated in less time than a month. The granulated flesh around it, therefore, generally covers this dead part of the bone before it is taken out.

fect may be known by occasionally pressing a probe upon the surface of the bone. For when the intended exfoliation is accomplished, the exfoliated piece gives way, on the pressure of the instrument. I do not, however, suffer it to be removed immediately on discovering its separation from the body of the tibia; but generally let it remain in its situation two or three days, in order to give sufficient time for its being completely disengaged, that it may be removed with less danger of being broken. I then complete the removal in the most gentle manner; sometimes by the forceps, sometimes by passing either the one side of a pair of forceps, or a director, under it, as circumstances may dictate.\* Having proceeded thus far, my attention is next directed to the removal of the loose pieces of bone within the cancelli. In almost every instance in which the process above described has been followed, I have found this bone at perfect liberty. But before I attempt its extraction, I always introduce a probe, in order to ascertain its exact position. In most of the cases which have come under my care, it is readily felt by the probe; but the practitioner should be prepared to meet with some disappointment here. There are cases, in which, from its smallness, it is not only extremely difficult to find the imprisoned piece, but even to discover any vestige of it, on the first examination. In three cases I found it so small as to require a very nice management of the probe to feel it; in two of these cases, the loose piece was found immediately after the enlargement of the aperture in the tibia was accomplished; in the third case, it was not found till two days afterwards. Out of the whole number of cases, which have occurred in my practice, I can advert only to one in which I have searched in vain for it: nor do I feel warranted to conclude, that even in this case

This, of course, obscures it from the view, but when loose, it is as readily extracted, with the loss only of a few drops of blood, as if it was perfectly exposed.

\* This exfoliation generally exhibits a very curious appearance. From an examination of seven or eight pieces of bone thus exfoliated, from as many different cases, it is evident that the ulcerative process, at the commencement of this disease, erodes the substance of the tibia, in this part, to such a degree, as to render the enlargement of the hole, by the caustic, more certain, than it would have been, had the tibia been of its usual thickness.



there was no such piece; but rather suppose, that, by its minuteness, it either eluded the search with the probe, or escaped unperceived in the dressings. This solitary instance occurred, before I was aware how very small some of these pieces are found to be. In the three cases I have mentioned, the loose pieces found would have escaped my notice, had I relaxed but a little of that perseverance with which I searched; and any conclusion against their existence, drawn from the disappointment, would, of course, have been erroneous.

"After having enlarged the aperture in the tibia, by the caustic process already described, and having found the loose exfoliation, I have in almost every case been able to extract it. There has, however, been an instance or two, in which it has been found still too large to pass through the aperture; although it could be moved upwards and downwards, in its bony case. In these cases I have been obliged to adopt the method already described in page 11; viz. of breaking the loose bone into two parts, in order to liberate it from its confinement. Had I not been able to do this, I must probably have been obliged to make use of the trephine.

"Having carefully examined all the pieces thus extracted in the different cases which have come under my observation, I have invariably found them to be exfoliations of the interior laminae of the tibia; no marks of affinity with the external surface of the bone having been found on any of them. I am likewise certain that none of the exfoliated pieces extracted from within the cancelli were any part of the exfoliation made by the caustic for the purpose of enlarging the hole in the tibia. The greater number of them were ascertained to be within the cavity of the bone, before any caustic was applied to the tibia. Besides which, I should inform the reader, that the pieces exfoliated by the caustic were removed with so much care, that no fracture of them was likely to occur; and if it had, it must have been perceived.

"After I have extracted one piece of bone from within the cancelli, I examine the bottom of the wound with a probe, to ascertain whether there be any more pieces: this I continue to do occasionally for about a fortnight, knowing that no cure can

be obtained if any portion of bone be left behind. The hole indeed would fill up with granulations, but a small oozing wound would remain. When the cancelli are cleared of these exfoliations, the wound should be dressed in the most superficial manner, and with the most simple dressings; applying compresses and a roller lightly over them. In the greater number of these cases, the skin is perfectly cicatrized in the course of six weeks, or two months, from the removal of the exfoliated pieces; and the inequality on the surface of the tibia is generally removed. The scar, indeed, and the external appearance of the leg, are somewhat different, after the cure in these cases, from what I have observed in most others. In a few instances, however, I have seen the wound continue open for several months, after the removal of the bones. In one case, indeed, it continued open nearly two years; during which time a probe could be passed within the cancelli, to the extent of four or five inches; but no material inconvenience accrued from it to the patient during this interval; and it afterwards healed up."

Under this treatment, the usual termination of the disease, with Mr. Whateley, has been in a speedy recovery. There have, however, occurred sometimes severe symptoms, which have not only endangered the limb, but have even destroyed the patient.

In two cases in middle aged women, in the intervals of child-bearing, the *infortunia* were most formidable, and the consequences extremely serious.\* They are attributed by the author, to a violent use of the forceps in extracting the exfoliations

\* These were violent inflammation, attended with great pain, extending itself through almost the whole of the tibia; and the formation of pus on the surface of its different sides, in almost every direction, as well as within the cavity of the bone. In one of these cases, the symptoms arose to such a height, as to destroy the patient in the course of a few weeks. In the other, the patient survived; but the irritation and inflammation were so great, for the first six months, as at length to leave the tibia eroded through its entire thickness, a little below the knee; where, on moving the limb, a kind of joint was observable. By the use of opium, fomentations, and emollient poultices, (the limb being kept in an horizontal position) the disease gradually abated; and after some large exfoliations from the substance of the tibia had been thrown off, the use of the limb was restored.



from the cavity of the *tibia*; but they are more fairly referrible to the patient's walking considerable distances while under surgical treatment, and serve as a proper warning to those who may have the management of similar cases; and will induce them to confine their patients to the house, if not to the bed, during the treatment.

The remaining part of the essay consists of twenty-two cases, with a plate and explanations.

The value of this pamphlet arises from its practical utility, and as affording additional information from that best source, an observance of nature, on a disease, though not of frequent occurrence, of great importance. If we differ in some points, possibly of opinion only, with Mr. Whateley, we admit in the fullest manner the service this species of medical literature is to the science; where, even if the hypothesis be wrong, the facts, if correctly observed and faithfully related, afford materials for a more legitimate theory, and more certain conclusions.

"This disease," says Mr. Whateley, "differs totally from those affections of the *tibia* produced by syphilis, as it does likewise from those usually denominated *NECROSIS*." There certainly are no facts that can induce us to suppose it has a syphilitic origin; but that it is a species or variety of the disease examined and so well described by Mr. Russel, (*Practical Essay on a certain Disease of the Bones, termed Necrosis*, 8vo. Edin. 1794), and by professor Weidman, (*De Necrosi Ossium, fol. Francfurti ed. Manum*, 1793), there is surely but little doubt. The disease, which these writers have described in the advanced period, Mr. Whateley has seen in the incipient state; or, possibly, in a mild variety, where only a small portion of bone has perished, but which being confined within a bony sheath, has kept up ulceration, until it has been removed. There are some particulars, however, in which the disease described by Mr. Whateley differs from the definition of *Necrosis*, as given by Mr. Russell. In Mr. Whateley's cases there seems to have been simply an exfoliation of the internal substance of the *tibia*, which, falling down among the cancelli and lying loose, keeps up an irritation and purulent discharge; while in the cases that

Mr. Russell denominates *Necrosis*, a covering of new osseous matter surrounds the dead bone. The period of attack is likewise different. Mr. Russell found *Necrosis* to be properly a disease of the early period of life. "I have never known," he says, (p. 92) "a case of it in which the attack began after the twentieth year, except in cases of *Necrosis* of the lower jaw. About the age of puberty, or from twelve to eighteen years of age, is the time of life at which patients are most liable to be attacked." Mr. Whateley found that in twenty-two cases, eighteen occurred to persons past thirty. We are disposed to think, from some cases that have fallen under our own particular knowledge, as well as from an examination of the numerous *sequestra* preserved in an anatomical museum of great extent, that Mr. Russell has too much contracted the period in which this disease attacks; and that its depredations in advanced life are not confined to the lower jaw. Professor Weidman gives a plate (*Tibula tertia*) that so much resembles the plate, and so closely accords with the description given by Mr. Whateley, that the identity of the diseases seems very completely established.

The mode of treatment here adopted for enlarging the opening in the tibia, through which to extract the *sequestra*, does not quite agree with the dexterity of modern surgery: neither does it appear to us adequate to the object. We assert this, subject however to the correction which the great experience of Mr. Whateley can well supply.

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*A Commentary on the Treatment of Rupture, particularly in a state of Strangulation.* By E. GEOGHEGAN. 8vo. pp. 95. London.

From the Medical and Physical Journal for 1810.

To deviate from the common path of the profession with safety and success, is not the work of ordinary minds; and he may justly be considered as the benefactor to his art who shall suggest any real improvement in the treatment of a disease which has occupied the attention and the pen of so many eminent writers. The interesting little work before us has, besides its intrinsic merit, a claim to our notice, in as much as



the subject was many years since opened in the pages of this Journal. In the year 1800, the author published some observations on this disease, in our fourth volume; and his farther experience has confirmed him in the validity of the opinions he then advanced. The present Commentary on the treatment of Hernia begins with a detail of the general directions given by different authors to accomplish the reduction by the hand; and after citing the opinions of Mr. Pott, Dr. Munro, Mr. Bell, Mr. Cooper, and Mr. Lawrence, the author says, (p. 28)—

“When I consider the talents, and the extensive opportunities which these writers possessed, it is not without anxiety that I contest their opinions; but experience has abundantly shown that they are ineffectual in practice; and I feel a conviction that they are unsupported by theory. I have given their own words that their full meaning may the more clearly appear, and that my inference may the more readily be understood.

“The first objects for our consideration are the structure of the parts concerned, the phenomena which the disease exhibits, and the pathology which may be fairly induced. When hernia has taken place in the groin, it has passed through two apertures: that which is next the abdomen is formed by the tendon of the transversalis and internal oblique muscles, and a fascia that lines the transversalis; the external aperture is formed by a separation of the tendinous fibres of the external oblique muscles; they are filled up with the spermatic chord in males, and by the round ligaments of the uterus in females; they form an oblique canal, which in old ruptures becomes straight, or nearly so. In crural hernia, it passes under Poupart's ligament, through a small aperture, in a tendon formed for the transmission of the femoral vessels. During a length of time it slips in and out of the abdomen uninterruptedly: a circumstance to which I would particularly direct the attention. Suddenly, after any great exertion of the body, such as lifting a weight, coughing, sneezing, &c. or external injury, it refuses to return into the abdomen. On examination it appears tumified, tense, and inflamed, accompanied with excessive pain, and derangement of the functions of the viscera, and universal dis-

turbance, the inflammation rapidly increases, the distention is prevented by the rigidity of the tendinous aperture, or by the neck of the sac which contains the intestine—hence strangulation.”

The author then goes on to state, what, however, has been stated by others; but the consequences which he deduces are his own, and important; that the strangulation and its symptoms are not produced by any altered state of the abdominal aperture, and that the inflammation and enlargement of the intestines are the essential points to be considered.

He then observes,

“From what has been stated, it appears that the indication of cure generally laid down, is not warranted by the nature of the disease, nor of the treatment, namely, to return the hernia through the apertures by pressure with the hands; because, as mentioned before, it is not the situation of the intestine, it is the enlargement of it, which occasions the symptoms; and that state should, in the first instance, forbid the attempt on account of its obvious impracticability.”

These remarks lead the author to, and prepare the reader for, a very material alteration in the treatment of strangulated hernia in its first stages, which we shall state in his own words.

“With the view of affording a satisfactory explanation of the practice, which I think ought to be pursued, founded on the principles which I have endeavoured to establish, I shall describe my own manner of treating a case, and the order which I think ought to be followed, and which has succeeded under my arrangement in, I believe, more than 36 cases since the year 1796.

“I place the patient in a recumbent position, with his shoulders a little raised to relax the trunk, but the pelvis not raised, as that would put the fasciæ on the stretch; the knees are to be drawn up. If the parts have not been irritated by handling them, or the body disturbed by jolting it about, or by any such roughness, I proceed directly to apply cloths, wet with cold water, expose the entire body to the open air, the doors and windows



being open. This practice usually succeeds within an hour.\* If it does not, I surround the hernia with my hand, or hands, at about its middle, in the way that I would grasp a gum elastic bottle, to press out its air or other contents, by gently approximating its sides, always holding in view that the tumor is to be emptied, and not pushed up,† and that a little assistance to the compressing force, which the coats of the intestine, (reacting from distention) are exerting, by lessening its area, even in a small degree, the air will be strongly impelled against the part of the tube which is closed, and through which it is only necessary that it should obtain exit, to effect our purpose. When it is small, as in femoral hernia, or in protrusions, which do not pass below the groin, or are elsewhere situated, it may be done with the fingers and thumb of one hand. Having applied the hands, I do not remove them for fifteen or twenty minutes, aware that reiterated impulses irritate, and that the effects of compression are lost each time that it is intermitted.

“Cases which have been long strangulated, and those which are accompanied with great pain and tension, are exceptions to this practice, so far as the employment of the hand, on the principle that the reaction of the intestine is at a pitch sufficient to propel the air through the coalesced tube, if its passage is possible, and that handling parts in such a state would endanger mortification or rupture of them. My chief reliance in such is on bleeding, proportioning the quantity by nearly the following scale:

“If the patient is young and robust, I take off sixteen ounces; if old, or weakened by previous illness, from six to ten ounces, should even debility‡ take place if attended with

\* In some cases where I could not immediately attend, I have directed that cold applications should be used until my arrival, and after an hour, they informed me that they were seized with a shivering; that they heard the wind rush out of the hernia; and that they were instantly relieved.

† I never press the hernia in any direction, or at all towards the aperture.

‡ It is very material in this, indeed in every disease, to discriminate between that debility which is the effect of pain, anxiety, and restlessness, and that which is the effect of impaired health or of original constitution. In the former, bleeding is advisable; in the latter it is generally improper to any extent.

great pain, I repeat the bleeding after three hours; but if the pain yields, I omit it; all this time I apply nothing topically, except cold water, and strictly avoid all attempts with the hand.

"I direct no medicine internally, and by these means I have succeeded in the instances before mentioned; and I am persuaded that they will scarcely ever fail, provided, that the taxis in the usual manner had not been practised, and that such other exasperating causes, as putting the patient with his head to the ground and his heels up, and jolting him about, had been omitted.

"Strangulated hernia, under this treatment, will in many cases continue several days without serious mischief; and in the event of the operation being required, peritoneal inflammation will be provided against.

"The tobacco clyster is the next remedy which I would advise; in a robust subject in the proportion of one drachm to a pint of water, which I would repeat of double that strength, if the first produced no effect; in an infeebléd person, half a drachm should be first tried.

"In the event of failure in our efforts to remove the strangulation, the operation of enlarging the apertures by the knife must be had recourse to; the precise time at which we should lay aside all other means, and commence this practice, is not easily decided on.

"In a young and full person suffering great distress, I would not defer it longer than six or eight hours, particularly if the hernia had been much handled, and also if bleeding had not been used; but where no mischief had been produced by external causes, and this evacuation had been premised, it might be deferred sixteen or twenty-four hours, always watching if the belly became sore to the touch, or much inflated, in which case to operate instantly. In an aged or debilitated subject it may be delayed longer. I have known it to continue in such, six, seven, or eight days, and afterwards to be removed without an operation."



Such is the treatment, and such the means which the author recommends in the strangulated hernia previous to operation. The points on which he differs from others are, chiefly, abstaining from violent attempts to reduce the gut, and objecting to the use of purgative medicines. He is the advocate of bleeding, of the application of cold, and, in short, of every antiphlogistic plan to reduce the size and inflammation of the intestine preparatory to its return. On the whole, we consider this commentary as a valuable addition to our information on a disease of great importance, both from its frequent occurrence, and the imminent danger which accompanies it; and we therefore strongly recommend the work to our readers.

## ORIGINAL PAPERS.

*On the Hydatids of the Uterus.*

By T. C. JAMES, M. D.

THE following case having some time since fallen under my notice, I shall, without any apology, venture to submit it to the press, with a few observations necessarily resulting from it.

Having been called to visit a patient who was supposed to be in the fifth month of her pregnancy, but who for several weeks previously had been distressed with uterine hemorrhage, at first in a moderate degree, but gradually increasing in violence until it became truly alarming. Upon my first seeing her, I was convinced from the severity of the pains which had come on some hours before, with regular intervals of ease, that abortion would inevitably take place, and thought it most safe to ascertain her precise situation, by an examination; by which I found the os uteri dilated to about the size of a half dollar, and a substance presenting very similar, as regarded the touch, to the placenta. Presuming that this was attached immediately over the os uteri, I thought it best to separate or perforate it with my finger, expecting that the liquor amnii and fœtus would then be immediately expelled: but, to my surprise, instead of these, there was discharged a mass something resembling a small placenta verging towards putrefaction with half a common wash-hand bason full of transparent spherical vesicles, from the size of a pea up to that of a small walnut, full of water: after which the hemorrhage ceased. I could not detect any appearance of the impregnated ovum or fœtus. The vesicles were connected with each other, and to the surface of the more solid mass resembling the placenta, by short pedicles or necks; they were extremely delicate, and easily broke down upon being handled. The woman recovered happily and speedily.

Mr. Home, in the second volume of the Medical and Chirurgical Transactions, relates the case of a young woman who died



in what was supposed the third month of pregnancy, where the ovum was filled with hydatids nearly of the same kind I have described; where, although the situation of the placenta was readily discovered, there was no appearance of fœtus or funis. This ingenious physiologist does not think it a change in the structure of the placenta, but a general affection of the amnion. When this disease takes place, as he observes, the natural healthy actions for the support of the fœtus are so much impeded that its growth is arrested; and when the patient does not early miscarry, the fœtus disappears; and in all the instances where miscarriage has taken place in a more advanced stage of the disease, no fœtus, as far as is known, has been found.

A case very similar in every respect is also related in the 2d volume of the London Medical and Physical Journal. The quantity of hydatids in this instance was measured, and found to be three pints and four ounces. In Mauriceau's and in Smellie's collections\* are also related corresponding histories of cases of uterine hydatids; and Morgagni, in his elaborate work *De causis et sedibus morborum*, after referring to a number of authors for similar cases, relates his having found numerous hydatids in the uterus of a bitch that died suddenly during a supposed pregnancy.†

A question might here occur, perhaps not readily to be solved, whether the hydatids are to be considered as the *cause* or *effect* of this singular morbid state of the uterus: whether they originate from the death of the fœtus and the destruction of the ovum, which I should rather presume to be generally the case; or whether, as in the liver of sheep and even that of the human species, they arise without any known or ascertained cause, and are to be considered as a peculiar original disease of the viscus itself. That this may sometimes be the

\* Mauriceau Observ. ccclxxvii. Smellie. Pt. II. No. 3.

† Ruysch in the 1st vol. of his valuable works has given two very curious and accurate plates of these hydatids of the placenta or uterus although he appears to be uncertain with respect to their nature; for at first he seems to have supposed them to be minute glands of the placenta degenerated into hydatids: and afterwards considers hydatids generally to be the extremities of sanguiferous vessels which have changed their original nature and degenerated into a morbid state. Ruysch Observat. Anatom. et Adversar. Anat.

case would appear from some facts related by Vigarous, a professor of Montpelier, who has of late years written on the subject, and who asserts that he had under his care the case of a woman who never obeyed the calls of nature, without discharging from the uterus more or less hydatids of the size of a large horse bean.

May we not suppose that many of the cases recorded by medical writers, under the title of dropsy of the uterus, and which are so frequently to be met with in nosological works, were in fact cases of hydatids of the uterus?

For a long time, it appears, that it was not determined from whence these hydatids had their origin, nor how they could be formed in such vast abundance in animals, until Hartmann in 1685 discovered them to be alive and to possess a peculiar structure and power of motion; and after him Dr. Tyson, an English physician, in a paper published in the 17th vol. of the Philosophical Transactions, proved very satisfactorily that they were a peculiar species of worm, which he termed *Lumbricus Hydropicus*. His investigations and experiments were on hydatids discovered in dissecting a gazella or antelope; and they appear, from his description, to be precisely the same as are frequently found in sheep and described by Dr. John Hunter and other writers. Tyson gives a plate of the worm of its natural size and magnified. His discovery excited the attention of naturalists; and Pallas, Hunter and others, having prosecuted the subject with considerable industry, confirmed the fact of their vitality.

In the course of their inquiries, they discovered in this worm new characters, which induced them to arrange it under the genus *tania*; and it is by Pallas called *tania hydatigena*. Linnæus's term is *hydradydatula*.

Notwithstanding this, writers have differed with respect to the vitality of these singular vesicles; and Tyson, although he acknowledged their vitality in the antelope, seems to doubt of the animation of those in the human subject that had fallen under his notice. But Pallas has attempted to do away this difficulty, by supposing that in the progress of their growth, and as their appendices, or necks, delineated by Tyson, are con-



tracted, and they assume more of the dilated and globular form, that they proportionally lose their vitality, till they are ultimately entirely deprived of both the one and the other on which the character of the *animal* depends; and they become mere inert and lifeless vesicles, though still frequently filled with a transparent or limpid fluid.

There appears, according to Dr. John Hunter, to be this difference between human hydatids and those found in sheep: that the former are found in great numbers in the same sac and of various sizes but have no neck or mouth, although he afterwards acknowledges that the mouth is not essential to the hydatids of sheep: and a late *Italian author*,\* who has written very fully on worms infesting the living human body, relates the case of a patient dying suddenly with symptoms of apoplexy, where, on examining the brain soon after death, a congeries of vesicular hydatids were found in each of the lateral ventricles, which he compares to small grapes suspended by a stalk. Each of these vesicles, upon being opened, was found to contain a worm with a spherical body full of water, and with a projecting neck and head not dissimilar to a species of *tania*. He has given a curious plate of this worm. In this case the disease must have been beyond the reach of medicine and death inevitable. Lieutaud also mentions two cases of hydatids being discovered after death in the meninges of the brain. *Hist. Anatom. Med.* vol. 2. p. 145. And the celebrated Ruysch asserts that he had met with a considerable number of hydatids in the plexus choroides. *Ruysch Adversar. Anatom.* p. 8.

I should have mentioned, that the very accurate Dr. Baillie, in his *Morbid Anatomy*, considers the hydatids found in the brains of sheep as exactly similar to those of the human liver, and inclines to the belief that the generality of the hydatids found in the human body, even those of the liver and of the uterus and placenta, are a sort of imperfect animals; and although there may be some difference between them in simplicity of organization, that this need be no considerable objection to the opinion, as life may be conceived to be attached to the most simple form of organization.

\* Valeriano Luigi Brera.

Dr. Hunter, above referred to, who has written a very ingenious paper on this subject, in the first volume of the Transactions of a Society for the Improvement of medical and surgical Knowledge, remarks, that of the numerous cases of hydatids related by writers, hardly any proved fatal whenever they found an outlet; so that whenever their existence is suspected, it must be of great consequence to endeavour if possible to procure them an exit or discharge. But when this cannot be effected, perhaps mercury used with some freedom promises to be as useful as any internal medicine that can be exhibited; and one case is related in a medical publication (*Medical Transact. of College of Physicians of London, vol. 2.*) where it appears to have produced a favourable effect exhibited in the form of calomel.

In this instance 135 hydatids were brought up from the lungs by coughing in the course of three or four months. They were of different sizes, from that of a pea to that of a pullet's egg. Cases similar in some degree are related both by Bonetus and Morgagni, Lieutaud and Baillie. May we not with Dr. Baker suppose, that in the above case the hydatids might have originated in the liver, and from thence, by the effects of disease found their way to some of the branches of the trachea.

It has been observed in rural affairs, that the hydatids that infest sheep are either prevented or destroyed by changing their pasturage from low and humid situations to a pure and dry atmosphere, on the sides of hills and mountains: and perhaps in the human subject, when there is a disposition to generate these vesicles, a similar change of air and situation may be attended with good effects, together with a generous and nutritious diet, easy of digestion, combined with a proper degree of invigorating exercise, and in fact a plan calculated to restore and support the healthy action of the vascular system generally, and that of the absorbents in particular.\*

\* To prevent the regeneration or new formation of hydatids or vesicles, Burns recommends introducing a bougie into the uterus or frequently injecting some astringent fluid into that viscus.



*Case of Singular Disease of the Uterus.*

By Dr. D. J. SWINNEY.

On the 27th day of March last, I was called upon to attend a female patient labouring under an excessive flow of the catamenia. This had continued for four days with scarcely any diminution: the discharge was also attended with a considerable quantity of whitish fluid approximating to the nature of half-formed pus.

On inquiry, I found a disease of the uterine system had existed to a troublesome degree, for about four months; and that for two weeks past only, the patient had been prevented from pursuing her usual employment as a servant in the family. I afterwards ascertained, that she had occasionally complained of it for more than ten years. She would sometimes tell her fellow servants that she felt something move in her body like a living creature. This only excited their ridicule without contributing to her relief.

When I saw her first she was very much debilitated from her menorrhagia, and felt excruciating pain in the region of the uterus. These symptoms were accompanied with severe retchings and vomiting, and an expectoration of tough phlegm; also with fever, dyspnœa, and want of appetite: headach, giddiness and pain in the loins; the extremities frequently very cold: these were attended with palpitations of the heart and faintings. Afterwards, by examination, I ascertained that there was a large abdominal tumor, which presented a soft feel to the fingers. This tumor was situated just below the umbilicus. Most of the symptoms were subdued by the use of the following remedies.

After some of the more common medicines in cases of vomiting had failed, and I had found that nothing remained in the stomach, I directed a large blister to be laid over the stomach, which very speedily checked the vomiting. By the use of rubigo ferri and the external application of flannels, kept constantly wet with cold vinegar, to the region of the uterus, the menorrhagia was subdued on the fourth day from the time I was called in. The debility was

in some degree removed by the use of a Peruvian bark bandage applied round the body. At this time also the fever entirely disappeared. The patient now lay much easier and seemed to inspire her husband and attendants with great hopes of her recovery; but on the fifth day the uterine region was again attacked with pains resembling labour-pains, which continued for two days, baffling the power of anodyne medicines. The patient from this time became restless; her jaws set; and her stomach so irritable that it would not retain any thing.

From existing symptoms on the sixth day, I concluded that a cancer or scirrhus uteri must be present. On the seventh, the pain subsided; the patient became speechless; and no relief could be procured but by injections and external applications. The patient appeared beyond the reach of the medical art. Thus she continued almost insensible until the ninth day, when she expired with very little pain.

Permission being obtained I opened the body, in the presence of a young gentleman,\* and found that the uterus was very much enlarged: that the cervix uteri formed a scirrhus tumor: that the body of the uterus contained at least three pints of yellowish purulent matter though not well formed; and that the os tincae afforded too small a passage for its free evacuation. I found also an adhesion between the anterior surface of the cervix uteri and the parts adjacent; this was easily separated by the fingers.

On examining the texture of the uterus it appeared no more than half the thickness of a healthy one and of a harder, firmer, membranous feel. Posteriorly there was discovered a small cavity in the uterus divided from a much larger one by a longitudinal septum: this septum was of a placental feel and appearance; and was attached both to the cervix and fundus. Within this cavity was contained about two gills of pure well-formed pus. In this situation and totally saturated with this fluid I found a mass of hair interwolved together forming a ball about the shape and size of a common hen's egg. This hair was of various lengths, from the eighth of an inch to that of nine inches and a half; and its colors were those of a light

\* Mr. Charles Busti.



brown and reddish sandy, and all shades between them as nearly as I can judge. This hairy mass is now in my possession.

There was also a circular surface of about three inches diameter in the fundus uteri, that was covered over with irregular scaly ossifications nearly close to each other, about the thickness of a common goose quill, some very small and others nearly half as large as the nail of the little finger. Their convexity was towards the internal surface of the uterus. The other contents of the abdomen showed not the least appearance of disease.

The woman when she died was about sixty years of age; had never had more than one child and that about her 37th year; had none by her last husband to whom she had been married about 14 years, in which time she had been once or twice attacked with symptoms of abortion and was delivered of an imperfect fetus. She was accustomed to hard labour as a servant in the kitchen of a large family, and was given to ebriety for several of the last years of her life.

I close this detail by submitting these facts to the gentlemen of the faculty for their candid consideration and investigation; for I am desirous that the cause of such a strange phenomenon may if possible be ascertained.

D. J. SWINNEY.

Blockley, May 15th, 1811.

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*An Account of a Case of Aneurism of the Femoral Artery, which was spontaneously cured.*

By JOSEPH PARRISH, M. D.

ELIZABETH VANDINE, aged about forty-three years, came under my care as a patient of the Philadelphia Dispensary, on the 14th of third month (March) 1810. She was suffering considerable pain from a tumor, situated on the inside of the left thigh, just above the knee, and extending along the course of the femoral artery, for several inches.

The patient informed me, that a few weeks before when carrying a bucket of water, she felt a sensation in the above



*ANEURISM of the FEMORAL ARTERY.*





mentioned part, similar to the sting of a bee; and soon after discovered a swelling which went on increasing, until she became alarmed and applied for assistance.

On examining the tumor, an obscure pulsation was perceptible; and from all the circumstances, I concluded it was an aneurism.

As she lived in a garret, in great poverty, admission into the Pennsylvania Hospital was proposed to her, to which she strenuously objected.

I directed her to keep the limb as quiet as possible, and used palliative treatment to allay pain. I continued to see her frequently, and marked the increase of the tumor with no small degree of anxiety.

She was visited at my request by Doctors Wistar, Physick, and Hartshorne; all of whom I believe supposed the disease to be aneurismal.

About a month after first seeing her, the tumor became very large and prominent. She experienced a great deal of pain and a smart sympathetic fever supervened, which required several bleedings. At last tired out with pain, and aware of her danger, she consented to be removed to the hospital, and was accordingly admitted on the 2d of fifth month (May) 1810.

Soon after her admission a consultation of the surgeons of the house was called by Dr. Physick; and Drs. Wistar and James accordingly attended. At this time it was easy to perceive that the tumor had diminished; and on examination Dr. Physick discovered that there was no pulsation in the anterior and posterior tibial arteries on the foot and ankle.

This very uncommon circumstance cast a shade of obscurity over the whole case; and it became difficult to decide on the true nature of the disease, and also on the plan of treatment which it would be proper to pursue. In that moment of uncertainty Dr. Physick offered an explanation, which the issue of the case has proved to be correct: he supposed that by a natural process a portion of the femoral artery had become imperious; and thus a cure had been accomplished. He therefore proposed that she should be kept quiet in bed; that moderate and equable pressure with a bandage should be made over the



whole limb; and that a long splint extending from the axilla to the foot should be so secured as to keep the limb and body fixed.

She continued in the hospital until the 11th of seventh month (July), when being desirous of returning home she was discharged.

After this I saw her but seldom until the 9th of fourth month (April) 1811, when she applied again to me at the dispensary. At this time she had regained the perfect use of her limb; but was complaining of a "beating in her breast," as she termed it. On examination I perceived a strong pulsation on both sides of the thorax; and suspecting her disease to be an aneurism of some of the great vessels contiguous to the heart, I commenced a plan of palliative treatment, which was pursued by one of my colleagues, Dr. Samuel Stewart, who attended in my place for a week preceding. On the morning of the 18th of the same month she died suddenly.

The husband positively refused the liberty of opening the body; but I prevailed on him to allow me to examine the thigh: and in the presence of Drs. Stewart and Elmer I dissected out the tumor, which is the subject of the accompanying engraving.

The aneurismal sac, at its posterior part, was attached to the os femoris; and the artery, as appears by the plate, was situated on the anterior part: the sac was filled with coagulated lymph of a dark chocolate color.

A little reflection on the situation of the sac and artery will give a ready explanation of the mode adopted by nature in the cure. The sac was attached to the os femoris: hence in the enlargement of the tumor, as it could not pass in a posterior direction, it was necessarily projected forward, and became, as was stated, very prominent: now the result of this was, that the artery above and below the sac, being retained in its usual situation by its natural investments, the portion of vessel connected with the sac was (as the latter became prominent) placed so completely on the stretch, that its two internal surfaces were brought into contact. Adhesive inflammation took place; and

for the space of four inches the cavity of this important vessel was rendered impervious.

Cases of the natural cure of aneurism are certainly rare; but I apprehend, they cannot with propriety be denied. A remarkable cure, which occurred in the carotid artery, may be seen in the "Transaction of a Society for the Improvement of medical and chirurgical Knowledge, vol. i, page 119. It is contained in a paper intitled, "Uncommon appearances of disease in the blood vessels, by M. Baillie, M. D. The carotid artery was the vessel involved in the disease; and a plate accompanies it.

In vol. ii. same work, page 268, there is another account of spontaneous recovery from aneurism, by John Major Wilson, house surgeon to the Westminster Hospital.

In our own country we have lately been presented with a case of natural cure, communicated by Dr. Lyman Spalding of Portsmouth, N. H. to Dr. Samuel Mitchell of New York, and published in the Medical Repository of that city. Hexade III. vol. i. No 4. page 348.

Some other detached cases of spontaneous cure of this formidable disease are to be met with in books less accessible, than those already quoted, but surely not of higher authority.

Now it is presumable that these facts are not merely interesting as matters of curiosity, but they go far to establish some important practical inferences.

Several modes of treating aneurism have been adopted by surgeons. Pressure, so continued as to bring the sides of the artery into contact, has been recommended. Scarpa admits the correctness of the practice of compression, when made above the aneurismal tumor and on a sound part of the artery: he forbids the use of it in cases of painful aneurism, also in those "attended with steatomatous ulcerated, earthy disease of the arterial coats." In cases favourable for compression, he asserts that bandages have produced radical cures of aneurism, and should not be entirely disused.

Other surgeons have used compression with a pad and tourniquet, and suddenly stopped the circulation. In the tumor this treatment is then continued until the limb becomes œde-



matous, when the tourniquet is directed to be removed; and the pressure of a pad and roller is said to be afterwards sufficient. The pain induced by sudden and powerful pressure is so great, that few patients can be found who will submit to it; and hence has arisen an objection to the practice.

Laying open the aneurismal sac and securing the artery, above and below it, by ligature has also been tried; but the danger of the vessel being in a diseased state, at the parts where the ligatures are to be applied, has formed a solid argument against this mode of treatment.

John Hunter, with his usual sagacity, perceived these difficulties: and, in order to remove them, he tied the artery on a sound part, above the sac, in a case of popliteal aneurism: adhesive inflammation was thus induced in the sides of the vessel; and its cavity was rendered impervious.

But even this great improvement has been found liable to objections: thus, for example, the sudden check to the circulation, through the limb, has in some few instances been so complete that, before the anastomosing branches could dilate sufficiently to carry the requisite supply of blood, mortification has taken place: and instances have occurred wherein the adhesive process did not go on; and, after the ligature separated, a dangerous hemorrhage arose from the femoral artery.

To meet the various difficulties and yet to fulfil the same intention, in cases of aneurism in any part of the femoral artery, long continued compression has been employed. Thus for example in the *London Medical Review*, for July 1810, under the head of medical and surgical intelligence, it may be seen that at Paris, in the *Hospice de l'Ecole de Médecine*, M. Dubois has employed compression on the thigh for popliteal aneurism, and has continued it even so long as 15 or 18 months. In the present account we are not informed of the exact place where the pressure was made, nor whether it was commenced in a very gradual manner.

The superficial situation of the artery in the groin renders it an eligible spot for the application of pressure; and, to insure success, it should be commenced in the most gradual manner. Thus if something could be contrived like the spring truss and

pad, with a screw adapted to it, half a turn might be made with the screw every day, until the requisite degree of compression was effected. In this manner it would be so gradual in its operation, that patients would be likely to bear it without difficulty; and it offers another very important advantage: as thereby the anastomosing vessels would have time gradually to enlarge, and thus carry on the circulation through the limb. For the latter plan I am indebted to Dr. Physick; who informs me that he intends to make trial of it in the first fair case that shall come under his care.



## INTELLIGENCE,

MEDICAL AND PHILOSOPHICAL.

## CURE OF HYDROPHOBIA.

*Extract from a Danish Gazette called "The Day," published at Copenhagen, for the month of October 1810.*

The counsellor of medicine, Dr. Schallern, of Bajreuth, in Germany, has lately, at Carolinereuth, on the estates of the grand hunter, Fr. Hardenberg, by the use of *Belladonna* and *Cherry Laurel water*, perfectly cured a girl four years old, who had been bitten by a mad dog. The disease was completely formed, the patient being averse to liquids.

It is thus proved, that there exists a remedy for this dreadful disease, even when it has already made its appearance.

*Interesting Fact respecting the Digitalis Purpurea.*

From the London Medical and Physical Journal, for 1808.

A fact, stated by Dr. Baildon in the Edinburgh Medical Journal, July 1807, may assist in explaining from whence has arisen the diversity of opinion on the action of the *Digitalis*. This gentleman observed in his own case, and repeated the experiment a great many times, that after the digitalis had taken effect, his pulse was not lessened in frequency when he stood erect; it was then upwards of a hundred. When he sat down, it fell considerably; when lying on his back, it fell much more. Thus, during the time it was at forty when lying, it was about seventy five when sitting, and above a hundred when standing. Doctor Baildon found the pulse to vary in this manner in all the patients to whom he gave the medicine in any extent. This is a fact of the most interesting practical nature in the medical history of digitalis; and calls for that minute attention on the part of the faculty, that shall either establish it as a principle in the exhibition of this powerful remedy, or reject it as one of those accidental effects that arise from inexplicable causes.

*Glostershire Vaccine Association, for promoting Cowpox and discouraging Smallpox Inoculation.*

THIS Association was formed in April, 1810. At present it consists of sixty-three medical practitioners of this county; all of whom, actuated by a high sense of public and professional duty, have individually and collectively declared, "*That they, considering their knowledge of Cow-Pox, do not believe themselves intitled either to practise or to sanction the use of Small-Pox Inoculation, and that henceforth they renounce it accordingly.*"

A general meeting was held at Gloucester, on the 6th of this month; at which it was resolved, that every attempt should be made to draw similar declarations from the medical practitioners in the different parts of the kingdom, because such unequivocal testimonies would most effectually tend to give confidence to the community, and to dispel those prejudices which are still artfully and ignorantly opposed to the progress of vaccination. To promote these objects, the following address was proposed, adopted unanimously, and ordered to be circulated as widely as possible.

TO THE PUBLIC.

We, the members of the GLOCESTERSHIRE VACCINE ASSOCIATION, having for the first time assembled together, take the earliest opportunity of declaring to the public the motives of our conduct. The facts which have induced us to adopt this measure, we trust will be received with attention by every one: they have carried conviction to our minds, and we anxiously hope that they may not be without effect upon the minds of others.

More than *twelve years* have elapsed since our illustrious countryman first made known the results of his accurate and successful inquiries into the powers of vaccination. His experiments have been repeated on the grandest scale; and every corner of the globe has borne testimony to their truth and their importance. By the means which he discovered, the most



pestilential and mortal distemper that ever attacked mankind, has already been banished from many parts of the world; and we believe that, by vigilance and attention, it may soon be eradicated from all. Every year's experience adds strength to this conviction; and we glory in it, as the highest claim that our art can exhibit to the gratitude and respect of our species.

In associating together to diffuse the blessings of a discovery which has already accomplished so much, we are not actuated by any fears respecting its ultimate and universal success. But we feel, that we should neither perform our duty as good citizens, nor as members of a profession to whom the care of the health of the community is intrusted, were we to remain inactive, while we see the murderous and disgraceful practice of smallpox inoculation prevailing around to an alarming extent.

Many of the causes which at present support an ill-founded opposition to the practice of vaccination, must soon pass away; and the next generation will probably wonder at the obstinacy which has been evinced by so large a proportion of our countrymen, in rejecting its great and well authenticated benefits, even after they had been gratefully acknowledged in almost every spot on the habitable world. This perverse spirit we sincerely lament: it is peculiarly discreditable to this country; and we deprecate it as most pernicious and unjust. Smallpox, in any shape, is a dangerous and loathsome disease. It is less fatal in the inoculation than in the natural state; but even this advantage has been purchased at a dreadful expense; thousands have been sacrificed, that a few might be saved.

It is our belief, that every argument for the continuance of this practice, has been removed. Upon this principle, we have associated together; and upon this principle we declare, that no man who has satisfied himself of the preventive powers of vaccination, *can be justified, either in practising or in sanctioning the use of smallpox inoculation.* To those, who, disbelieving in the efficacy of cowpox, have resisted the strongest testimony that ever supported any medical question, we have nothing to say. We are sorry that, on a subject of this kind, any individuals do not feel as we earnestly wish that all should

feel. But we can neither impeach their integrity, nor reprobate their inconsistency. To those, however, who, knowing that they possess an antidote to the ravages of smallpox, and yet become accessary to the propagation of that distemper, another language ought to be spoken. Our meeting this day, and the whole of our previous proceedings, are meant to indicate our *abhorrence* of such conduct, because we consider it as the destructive triumph of avarice or timidity, over professional integrity and independence. This most criminal indecision, has, in our opinion, prolonged the existence of prejudices which might long ago have disappeared. Medical men, who know, and have admitted, the advantages of vaccination, are at least bound to act in conformity with that knowledge. A conscientious and explicit avowal of their sentiments, supported and enforced by a manly and consistent line of conduct, must inspire confidence into the public mind, and facilitate the advancement of a cause, for the success of which every good man must be interested. We have attempted to do so; and we most fervently hope, that our endeavours may not be in vain.

But while we thus express our opinions of the duties which ought to bind us, and the rest of our brethren, we must at the same time remark, that our best exertions will be fruitless, unless the public feeling upon these points in some measure corresponds with our own. While medical men are held responsible for their professional acts, it is despotism to attempt to awe them into practice which their reason and their conscience condemn. We feel ourselves called upon to resist such influence; and we know, that, by so doing, our claim to public confidence and esteem should not be diminished. If we are required to follow the honest decisions of our judgment, in the ordinary transactions of our profession, much more is it necessary for us to do so, in a case which involves consequences like the present.

Did those who employ smallpox inoculation expose their own children only to the dangers of that disease, we might justly bewail their fatal ignorance and obstinacy; but when, in addition to this, they involve a whole district in the same perils, they are *guilty of cruelty and injustice*, and are answerable for evils, the extent of which no human being can calculate. We dare not,



with our present knowledge of cowpox, become actors in this work of death; and we feel the greatest concern, that the question respecting the dissemination of a plague, should lie at the mercy of any individual.

Were the evidence in favour of cowpox infinitely less perfect than it is, the circumstances which we have stated will always claim the most serious consideration from every benevolent mind: and when they shall have been as accurately and extensively examined as they should be, we confidently expect a determination in unison with our wishes. Should our attempts to accelerate this event prove unsuccessful, we shall at least enjoy the satisfaction of believing that we have not neglected the means which have been placed within our reach, of conferring a substantial benefit upon our race; and we know that they are at this instant greater, than ever were possessed by the members of our profession at any former period.

In conformity with the views just stated, it was further resolved, at the meeting above referred to, that measures should be taken to request the cooperation of the members of the medical profession throughout the kingdom, for the purpose of checking the unlimited dissemination of smallpox contagion; that a central vaccine institution be established at Gloucester, as soon as sufficient funds are collected; and that subscriptions be opened at the banking-houses of Gloucester and Cheltenham.

The members of this association, in presenting this abstract of their proceedings to the public, earnestly beg for that assistance and support which they think themselves intitled to expect. They confidently appeal to every individual, of every denomination, who either has experienced or wishes to experience the blessings of vaccination, cordially to unite with them in forwarding this great cause. Considerable expense has been already incurred, and the purposes of the association cannot be carried on without incurring more. It is hoped, therefore, that aid in proportion to their importance will be freely bestowed and that the inhabitants of a country, which has given to the world the most valuable of human inventions, will not be backward in contributing to diffuse its advantages.

Gloucester, July 25, 1810.

EXTRAORDINARY EFFECTS OF THE EFFLUVIA FROM  
QUICKSILVER.

*Extract of a letter from Lisbon, dated 12th May 1810, giving an account of a singular effect of the fumes of Quicksilver, in the crews of two ships on that station.*

A LARGE quantity of quicksilver was saved from the wreck of a Spanish ship about Cadiz, by two or three of our ships, and was placed in their spirit rooms. An alarming illness broke out among the crews, all of whom were more or less salivated. The surgeons, pursers, and three petty officers, who were nearest the place where it was stowed, felt its effects the most, their heads and tongues having swelled to the most alarming degree. We have been under the necessity of sending the *Triumph* to Gibraltar to be cleared, and to have the people sent to the hospital, this ship having been most affected, having had the greatest quantity of quicksilver on board. The Phipps schooner has been cleared in this harbour, by boring a hole in her bottom, to allow the quicksilver to run out. Every rat, mouse, and cock-roach on board, has been destroyed, and the symptoms of general salivation have appeared in a strong degree. It may be a question whether this effect of the quicksilver was owing to its having lain long in sea-water, or whether the rotting of the leather bags which contained it might have a share in producing these *effluvia*. It is in favour of the latter opinion, that the officers above mentioned, who slept over the bags were most affected, whereas those who slept near the quicksilver, which run lose, were not affected. This was particularly observable in the Phipps schooner, where the men slept on the cables, close to where the quicksilver had flowed in consequence of escaping from the bags, and yet they were but little affected in comparison of the others.

The editors are also informed, from undoubted authority, that three surgeons of the fleet were directed to meet, and after investigating this matter, to report upon it, and that their report confirms the particulars of this statement.



*College of Physicians.*

At a stated meeting of the College of Physicians of Philadelphia, held July 2, 1811, the following officers were elected for the ensuing year:

*President*,.....Dr. Adam Khun,  
*Vice-President*,....Dr. Samuel Duffield,  
*Censors*,.....Dr. Thomas Parke,  
                             Dr. William Currie,  
                             Dr. Samuel P. Griffiths,  
                             Dr. Caspar Wistar,  
*Treasurer*,.....Dr. Thomas C. James,  
*Secretary*,.....Dr. Thomas T. Hewson,

---

*University of Pennsylvania.*

At a Commencement in the University of Pennsylvania, held the 26th April, 1811, the degree of Doctor of Medicine was conferred on the following gentlemen:

Samuel Ayers, of *New Hampshire*.

John Y. Lansing, A. H. Stevens, Samuel Shuts, of *New York*.

William S. Coxe, William Elmer, of *New Jersey*.

Adam Hays, Jonathan Murdock, Samuel Nixon, John Reynolds, George Schott, of *Pennsylvania*.

Joel Lewis, Allen M<sup>c</sup>Lane, of *Delaware*.

John Austin, John H. Briscoe, William Hammond, John D. Perkins, William Purnell, Robert J. H. Purnell, J. D. Rea-del, James Stewart, Matthew Vanlear, of *Maryland*.

William S. Alexander, T. B. Anderson, G. H. Batte, R. L. Bohannen, John Briscoe, William Buchanan, Henry Chambers, M. Clarke, Hawes N. Coleman, Abner Crumpe, Ashley Davis, Charles Edwards, William French, Joel W. Flood, T. Goode, P. E. Graves, John Gilliam, F. J. Mittaner, James Marye, Samuel Meny, John Roanes, J. M. Sheppard, P. B. Tindall, J. Trent, J. Whitlow, A. L. Wooldridge, of *Virginia*.

Benjamin Ballard, Thomas A. Cabarrus, Stephen Davis, of *North Carolina*.

H. C. Glover, A. Garden, H. M. Holmes, Joseph F. Lees, A. De Leon, William M<sup>c</sup>Caw, N. Starke, F. Y. Porcher, of *South Carolina*.

William Hughes, Joseph M'Murtrie, of *Kentucky*.

Edward Grant, of *Tennessee*.

John Kee, of *Mississippi Territory*.

Robert Abbott, of the island of *St. Croix*.

And on May 30th the degree of Doctor of Medicine was conferred on Thomas C. James, bachelor of medicine and professor of midwifery in the University of Pennsylvania; and on Elias W. Napier, of Kentucky.

---

*Pennsylvania Hospital.*

At a meeting of the managers, held at the Hospital May 13, 1811, the following contributors were chosen

PHYSICIANS.

Thomas Parke,

Benjamin Rush,

Philip Sing Physick,

Benjamin S. Barton,

John S. Dorsey,

Joseph Hartshorne, and

Thomas C. James, was elected physician to the lying-in department; and

Samuel Calhoun, physician to the out-patients.

---

*Humane Society.*

MANAGERS OF THE HUMANE SOCIETY OF PHILADELPHIA.

Elected March 1811.

Joseph Cruikshank, *President*,

Isaac Snowden, *Secretary*.

William Leedom,

Matthew L. Bevan, } *Inspectors.*

Dr. Samuel P. Griffiths, }

Thomas C. James, } *Committee of*  
Joseph Parrish, } *Correspondence.*

Charles Marshall,

Benjamin Thaw,

Samuel Pancoast, jun.

John Bacon,

Joseph Hornor, *Treasurer.*



*Thermometrical Observations.*

State of the weather at Philadelphia, for the first six months of 1811.

## JANUARY.

<i>Thermometer.....</i> Lowest,	-	-	-	-	20
Highest,	-	-	-	-	53
Mean,	-	-	-	-	30

*Winds.....*NE and NW, generally.

This month, on the whole, moderate weather; there being only occasional obstructions to the navigation from the ice, yet changeable and unpleasant. Not much snow: no sleighing.

## FEBRUARY.

<i>Thermometer.....</i> Lowest,	-	-	-	-	13
Highest,	-	-	-	-	48
Mean,	-	-	-	-	30

*Winds.....*Northerly.

Weather moderate; though changeable. No severe weather till 19th, then real winter weather till near the end of the month.

## MARCH.

<i>Thermometer.....</i> Lowest,	-	-	-	-	28
Highest,	-	-	-	-	69
Mean,	-	-	-	-	40

*Winds.....*Chiefly easterly.

The month was remarkably mild: scarcely any stormy weather.

## APRIL.

<i>Thermometer.....</i> Lowest,	-	-	-	-	35
Highest,	-	-	-	-	70
Mean,	-	-	-	-	48

*Winds.....*Mostly easterly.

Moderate, but changeable: not remarkable for heat or cold.

## MAY.

<i>Thermometer.....</i> Lowest,	-	-	-	-	46
Highest,	-	-	-	-	76
Mean,	-	-	-	-	60

*Winds.....*Variable. More westerly and southerly.

A changeable month: cool for the season. Spring more forward than usual; probably owing to the dry warm weather in March.

## JUNE.

<i>Thermometer.....</i> Lowest,	-	-	-	-	62
Highest,	-	-	-	-	86
Mean,	-	-	-	-	70

*Winds.....*More westerly and southerly.

Some warm weather in this month, and some unusually cold: on the whole fine weather.

## FOREIGN PUBLICATIONS.

## NEW WORKS AND RECENT EDITIONS.

*Practice of Medicine.*

AN Inquiry into the Causes producing the extraordinary addition to the number of Insane. By William Saunders Haslam, M. D. 8s. 6d.

Illustrations of Madness; exhibiting a singular Case of Insanity, and a no less remarkable difference of medical opinions. By John Haslam, M. D. 5s. 6d.

Additional Cases; with further directions to the faculty, relating to the use of the humulus, or hop, in gout and rheumatic affections. By A. Freake. 8vo. 1s. 6d.

Medical Histories and Reflections. By John Ferriar, M. D., senior physician to the Manchester Infirmary, Dispensary, Lunatic Hospital, and Asylum; second edition, corrected and much enlarged. 3 vols. 8vo., 1l. 4s.

*Surgery.*

Surgical Observations, Part III. On Injuries of the Head, and miscellaneous subjects. By John Abernethy, F. R. S. 8vo. 7s.

Practical Observations on the Sclerocoele and other morbid enlargements of the Testicle; also on the causes and cure of the acute, spurious, and chronic Hydrocele. By Ths. Ramsden, surgeon to Christ's and the Foundling Hospitals, and assistant surgeon to Bartholomew's Hospital. 8vo. 7s. 6d.

An Essay on some of the stages of the operation of cutting for the stone. By C. B. Trye.

*Anatomy.*

A System of Anatomy. By John Gordon, M. D. Lecturer in Anatomy and Physiology. Fellow of the royal college of surgeons. Edinburgh. 4 vols. 8vo.

*Chemistry.*

Outlines of a Course of Lectures on Chemistry, applied to the Arts and Manufactures. By F. Accum. 12mo.



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The Principles of Botany and Vegetable Physiology. Translated from the German of D. C. Willdenow. A new edition, with the author's last corrections and improvements.

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Memoirs of the Wernerian Natural History Society, for the years 1808, 9, 10, with fifteen engravings. 1l. 4s.

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Remarks on the Nomenclature of the New London Pharmacopœia. By John Bostock, M. D. 2s. 6d.

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Reports of the Utility and Employment of Mercury in the treatment of Inflammatory and other Diseases. By Sir George Alley.

Transactions of the Geological Society. London. Vol. I.

An Inquiry concerning Injuries to the Canal of the Intestines; illustrating the treatment of penetrating wounds and strangulated hernia. By Benjamin Traverse, demonstrator of anatomy at Guy's hospital, surgeon to the hon. East India company, and to the London infirmary for the diseases of the eye.

## RECENT AMERICAN PUBLICATIONS.

The American Medical and Philosophical Register; or Annals of Medicine, Natural History, Agriculture and the Arts. Conducted by a society of gentlemen. Vol. I. New York. E. Sergeant.

This work, the first volume of which is now completed, promises to make an important addition to the science and literature of the United States.

A Dissertation on the Proximate Cause of Inflammation, with an Attempt to establish a Rational Plan of Cure. Submitted, &c. for the degree of Doctor of Medicine. By Alexander H. Stevens, A. M. of New York, honorary member of the Medical Society of Philadelphia, &c.

An Inaugural Dissertation on Insanity. Submitted, &c. for the degree of Doctor of Medicine. By Theodoric Romeyn Beck, A. M. licentiate in medicine of the Medical Society of New York.

An Essay on the Connexion of Life with Respiration in New-born Animals, or an Experimental Inquiry into the Phenomena attending the Life and Death of Animals brought forth in different media. By William Purnell of Maryland, Member of the Philadelphia Medical Society.

The Baltimore Medical Lyceum. Edited by Nathaniel Potter, M. D.

The first number of this journal has just appeared. It contains a variety of interesting matter, and fully vindicates the industry and talents of the editor.

*Proposed American Republications.*

A System of Chemistry. By Thomas Thompson, M. D. F. R. S. E., and lecturer on chemistry, Edinburgh. From the 4th London edition. Thomas Dobson, Philad.

A Treatise on Ruptures; containing an anatomical description of each species, with an account of its symptoms, progress and treatment. By William Lawrence, member of the royal college of surgeons, and demonstrator of anatomy at St. Bartholomew's hospital. From the second edition, considerably enlarged and improved. Edward Parker, Philad.

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